2019

Essays on Occupational Regulation and Paid Sick Leave

JIE Chen
Lehigh University, chenjie0410@hotmail.com

Essays on Occupational Regulation and Paid Sick Leave

by

Jie Chen

Presented to the Graduate and Research Committee

of Lehigh University

in Candidacy for the Degree of

Doctor of Philosophy

in

Economics

Lehigh University

August 2019

Approved and recommendation for acceptance as a dissertation in partial fulfillment of the requirements for the Doctor of Philosophy.

Date

Committee Members:

Chad Meyerhoefer
Committee Chair

Robert Thornton
Committee Member

Seth Richards-Shubik
Committee Member

Edward Timmons
Committee Member

Acknowledgments

During my doctoral studies I have benefitted from the encouragement and support of many people. I am especially grateful to my advisor, Professor Chad Meyerhoefer, for his invaluable insight and encouragement. I am also very grateful to Professor Edward Timmons for providing valuable resources and support. Professor Robert Thornton and Professor Seth Richards-Shubik provided insightful feedbacks, and their approaches to research have left a lasting mark. My research was supported in part by the Knee Center for the Study of Occupational Regulation and I appreciate the support from all the CSOR staff. My family has long encouraged and supported my academic pursuits, and their support has been instrumental in my personal and professional life.

Table of Contents

List of Tables

List of Figures

Abstract

This dissertation consists of three essays. The first essay examines how the change in training requirements for certified nursing assistants influences the staffing hours and quality of care in nursing homes. In particular, I use the 2000-2016 Nursing Home Compare data on staffing information and quality of care in nursing homes and seek to evaluate certification requirements on certified nurse assistants (CNA). The impact of CNA training is identified by exploiting state-level variation in required total training hours and the ratio of clinical to total training hour requirements across states. Results show that a higher ratio of clinical to total training hours is associated with an increase in staffing hours of nurse assistants and a decrease in licensed practical nurse hours, as well as improvement in quality of care. These effects are more pronounced among large nursing homes and nonprofit nursing homes.

The second essay investigates the effects of dental hygienist scope of practice regulations and autonomy levels on dental care access, utilization, and expenditure. I measure the strength of these laws by extending the Dental Hygiene Professional Practice Index to the years 2001 to 2014. Data on dental care utilization for this analysis come from the 2001-2014 Medical Expenditure Panel Survey. Using a difference-in-difference approach that exploits variation within states over time in scope of practice laws for identification, I find evidence that increasing the autonomy level of dental hygienists modestly increases dental care utilization, on average. However, increases in use are more pronounced in areas with a shortage of dental care providers.

In the third essay, I estimate the short-term effects of paid sick leave on worker absenteeism and health care utilization in the U.S. using data from the 2000-2013 Medical Expenditure Panel Survey. In order to account for nonrandom selection into jobs that offer paid sick leave benefits, I use a difference-in-difference matching specification and estimate the

treatment effect of paid sick leave separately for workers who gained paid sick leave and workers who lost paid sick leave. I find that losing paid sick leave benefits decreases the probability of taking sickness absence days among both male and female workers, but that gaining benefits increases absenteeism only among female workers. I also find that the probability of having an outpatient medical visit is higher among women who gain paid sick leave, suggesting that expanding paid sick leave to more women could be welfare improving.

Chapter 1 Do Training Requirements of Certified Nursing Assistants Affect Staffing Decisions and Quality of Care in Nursing Homes?

1.1 Introduction

The quality of care received by residents in nursing homes depends on the staffing of nurse assistants. As of 2016, about 1.35 million elderly and disabled individuals in the U.S. reside in nursing homes (Kaiser Family Foundation, 2018), where over 80 percent of the direct care work is provided by certified nurse assistants (Cawley et al., 2006; Pennington et al., 2003). There has been considerable research on the impact of nurse staffing on care quality in nursing homes (see Backhaus et al., 2014 for a review), but only a few studies have focused on nurse assistant staffing. In those causal studies, researchers have exploited changes in macroeconomic conditions (Konetzka et al., 2018), labor market conditions (Cawley et al., 2006), and nursing home regulations (Lin, 2014; Lu, 2012; Lu and Lu, 2017; Matsudaira, 2014) to identify the policy impact on nurse assistant staffing.

Recent research has shed new light on how quality of care in nursing homes is related to the training requirements of certified nursing assistants. In 1987, the federal government mandated that all CNAs receive a minimum of 75 hours of initial training with a minimum of 16 hours of supervised practical training (Department of Health and Human Services, 2002). States were granted the authority to require additional training hours for CNAs. Trinkoff et al. (2013) linked data on regulations for CNA training requirements across states including required clinical hours, total initial training and in-service hours to the 2004 Nursing Home Compare data with several Quality Indicators for resident outcomes including pain, antipsychotic use, falls with injury, depression, weight loss and pressure ulcers. They report that in 2004, nursing homes in states that require clinical training hours above federal minimums had significantly higher quality of care than those in the states that require federal minimum training hours. Trinkoff et al. (2017) implemented the same design using the 2010 data and found similar results – higher required CNA training hours were associated with better resident outcomes in nursing homes. Nevertheless, since both studies use only cross-sectional variation across states, the estimates are biased if there are unobserved state and patient characteristics that are correlated with health care environments. Other cross-sectional studies on state CNA training requirements examined their relationship with CNA job satisfaction (Han et al., 2014) and CNA training cost (Tyler et al., 2010). However, none of those studies have examined the impact of CNA training requirements on staffing of nursing assistants and other nurses in nursing homes.

In this study, we explore the variation in training requirements across states over a long time period (2000-2016) to estimate the causal effect of CNA training requirements on both staffing hours and quality of care in nursing homes. In particular, we use the variations in the required total training hours and the ratio of clinical to total training hour requirements due to the fact that seven states increased their mandates for CNA total training hours over our sample period. Our dataset is the 2000-2016 Nursing Home Compare (NHC) data, which have information on staffing and quality of care. Staffing variables in the NHC are measured as hours per resident per day for each type of direct care workers – nurse assistants (NA), licensed practical nurses (LPN), and registered nurses (RN). To measure quality of care in nursing homes, we use Quality Measures (QMs) from the NHC.

Our results show that an increase in the ratio of clinical to total training hours leads to an increase in staffing hours of nurse assistants and a decrease in licensed practical nurse hours, as well as improvement in quality of care for residents with active daily activities, weight loss, pain, and/or a catheter. We find more pronounced effects among large nursing homes and nonprofit nursing homes.

This study makes three main contributions. First, unlike studies in the nursing literature that focus on the impact of CNA training requirements on individual nurses' job satisfaction or quality of care, we assess the training's impact on the employment of nurses/assistants in nursing homes. To the best of our knowledge, this paper is the first to evaluate the impact of nursing assistant training on healthcare facilities' staffing decisions. Our findings provide evidence that nursing homes are responsive to increases in clinical training requirements and that they do not change the total employment of direct workers in response to the requirements but shift from the most expensive registered nurses to nursing assistants. Second, previous findings on the relationship between CNA training and nursing home care quality rely on cross-sectional data; therefore these correlations may not reflect a causal relationship. In this study, our identification exploits variation in state policies over time, which enables us to provide better evidence on the impact of CNA training on patient outcomes and staffing decisions. Unlike Trinkoff et al. (2013) and Trinkoff et al. (2017), we find that increasing total CNA training hours is not as effective as increasing the clinical hour ratio in improving care quality in nursing homes, nor in changing the staffing mix. Relatedly, the effects of clinical training ratios are consistent – positively associated with care quality. Third, we shed light on how staffing decisions influence quality of care in health care facilities. Increasing nurse assistant staffing could potentially improve care quality pertaining to direct care work. Previous causal studies on the relationship between nursing home staffing and care quality tend to find a strong impact of licensed practical nurses (LPN) and registered nurses (RN) but no effect of nurse assistants (Lin, 2014; Matsudaira, 2014). However, we find that improvements in quality of care could also be driven by more employment of nursing assistants with higher training quality.

1.2 Background

1.2.1 Role of CNAs in nursing homes

Certified nursing assistants (CNAs), also known as nursing aides, help patients with daily living tasks. CNAs work primarily in nursing homes, though some may work in hospitals. About 80 to 90 percent of the care work in nursing homes is provided by CNAs (Cawley et al., 2006). Demand for long-term low-skilled health care workers such as CNAs is rising due to an aging population covering higher demand for nursing home care. However, the turnover rate of CNAs in nursing homes is alarmingly high with a range from 70 to 100 percent annually (Howe, 2014). There has been a shortage of low-skilled nursing aides due to the attrition of people leaving the profession due to general dissatisfaction, a potential lack of respect, and low wages (National Center for Health Workforce Analyses, 2004). Neverthless, longer required training hours for CNA has been found to be related with higher job satisfaction among CNAs (Han et al., 2014).

1.2.2 Minimum training requirements

To earn a CNA certification, one needs to pass a state-issued competency exam after the completion of a certificate program with 6 to 12 weeks of coursework at a community college or medical facility. The certificate programs are often referred to as nurse aide training and competency evaluation programs (NATCEPs, Tyler et al., 2010), which were established in 1987 by the passage of the Omnibus Budget Reconciliation Act (OBRA 1987). Under the federal rules, NATCEPs were prescribed with certain minimum features, including a basic curriculum, a minimum of 75 initial training hours with a minimum of 16 supervised practical training hours, and certain other competency requirements. In a classroom setting, CNAs are trained with all aspects of patient care, through lectures as well as hands-on demonstrations. Clinical training is usually instructed by nurses and medical professionals in a long-term care facility. During program, nurse aides get the knowledge of certain important topics such as anatomy and physiology, basic patient care, patient's rights, infection control, measurement of vital signs, etc. The average nurse aide program cost for one credit hour is about $71.50.[1]

Even though there has been no change in the federal minimum requirements on CNA training since 1987, state regulations vary significantly and are constantly evolving. As of 2016, 31 states (including DC) have extended the minimum number of training hours beyond 75 hours to as many as 180 hours, among which 14 states (including DC) require a minimum of 120 or more training hours (Paraprofessional Healthcare Institute, 2019). According to Trinkoff et al. (2017), the number of states requiring extra initial training hours beyond federal minimums increased to 31 in 2010 from 26 in 2004.

1.3 Data

Our main source of data is the 2000-2016 Nursing Home Compare (NHC) data from Centers for Medicare & Medicaid Services (CMS), covering over 13,000 Medicare and Medicaid certified nursing homes operating in the U.S. The database updates the information on a monthly basis and provides nursing home characteristics including basic operational information – number of beds, payer types, occupancy rates, etc. – as well as quality of resident care and nurse staffing information.

[1] This information is taken from CNA Buzz: https://www.cnabuzz.com/cna-guide/cna-training-cost/.

The staffing data in NHC contain how the nursing home is staffed with different types of nurses – director of nursing, licensed practice nurse (LPN), registered nurse (RN), and nurse assistant (NA). Each facility reports the number of full-time equivalent hours (FTE, including full-time, part-time, and contract nurses/assistants) for each type of nurse for a 2-week period prior to each survey date. CMS then converts the FTE to nurse staffing hours per resident day (HPRD) using the following formula (Centers for Medicare and Medicaid Services, 2017):

$$\text{HPRD} = \frac{\text{FTE}\times 70}{14\times\text{Number of total residents}}.$$

The FTE information is reported separately for licensed practice nurses (LPN), registered nurses (RN), and nurse aides (NA).[2] Since the formatting of staffing data released in the NHC changed in 2011 from reporting risk-adjusted *HPRD* to reporting only *FTE*, we rely on data from Brown University's LTC Focus to obtain more reliable measures of nurse staffing.[3] We evaluate the impact of the CNA policies on nurse aide hours per resident day as well as LPN hours and RN hours.

The quality of care in nursing homes is measured using the Quality Measures (QMs) from the NHC Minimum Data Set (MDS), which collected facility-level residents' outcomes including physical and cognitive status, acute medical condition and behavioral and emotional status. To create a comprehensive view of care, MDS QMs are generated annually, using quarterly data on resident care outcomes and other parameters reported by nursing homes (RTI

[2] RN hours includes registered nurses, RN director of nursing, and nurses with administrative duties; LPN hours includes licensed practical/licensed vocational nurses; Nurse Aide hours includes certified nurse aides, aides in training, and medication aides/technicians. (Centers for Medicare and Medicaid Services, 2017)
[3] LTC Focus files were supplemented from several data sources: Online Survey Certification and Reporting (OSCAR) data, Nursing Home Compare (NHC) and Area Resource Files (ARF). See ltcfocus.org.

International, 2012). Based on quality measures used in other studies (Cawley et al., 2006; Lu and Lu, 2017; Matsudaira, 2014; Nakrem et al., 2009; Trinkoff et al., 2013, 2017) and data availability throughout the entire sample period, we focus on the following resident outcomes: (1) percentage of residents whose need for help with daily activities has increased after admission (*ADL*); (2) percentage of residents who self-report moderate to severe pain (*Pain*); (3) percentage of residents who have/had a catheter inserted and left in their bladder (*Catheter*); (4) percentage of residents who lose too much weight (*WeightLoss*). Since these resident outcomes are in percentage terms, we use the logarithm of the odds ratio for each outcome by transforming it into $\ln(P_i/(1-P_i))$ where P_i is the percent of residents in nursing home *i*. As no value is assigned when P_i is zero or one, we recode each resident outcome as 0.0001 for zero value and as 0.9999 for one.

Our initial sample contains 270,446 observations from 2000 to 2016. To avoid large fluctuations in staffing decisions, we restrict our sample to 13,565 nursing homes that were present each year between 2000 and 2016 (*N*= 230,605).[4] We then drop 4885 observations with zero or extremely high staffing levels.[5] Since quality measures are only available after 2005, the sample for quality analysis includes 149,781 observations with non-missing quality measures from 2005 to 2016.

We compile data on state requirements for CNA training hours from several sources. The Office of Evaluation and Inspections (2002) visited or surveyed 48 states' nurse aide training programs and reported the CNA training requirements in those states as of the year 2001.

[4] The staffing ratios of a new facility are often very unstable as the demand rises in the first few years of operation (Matsudaira 2014).

[5] Based on the distribution of staffing variables, we restrict our sample to NA hour<13.8, RN hour<6.0, and LPN hour<6.9.

Similarly, the Iowa CareGivers Association (2004) surveyed 44 states and collected the state regulations for 2004. We also collect the 2006 regulation data from Hernandez-Medina et al. (2006) and the 2009-2016 regulation data from the Paraprofessional Healthcare Institute (2019). In addition, we rely on state statutes and regulations to verify the timing of the changes in training hours and to correct some mistakes.

Table 1.1 shows how the required CNA training hours changed from 2000 to 2016 for all the states. During the period from 2000 to 2016, seven states increased required total training hours with the largest increase from 75 to 120 hours. Among the seven states, two (Maine and Wisconsin) also increased required clinical training hours. As of 2016, there are still twenty states only requiring federal minimum training hours for CNAs.

1.4 Empirical model

We use a difference-in-differences design to evaluate the impact of CNA training requirements on nursing staffing and quality of care in nursing homes. In particular, the effect is identified by comparing nursing homes in the states that experienced changes in requirements for CNA training hours to their counterparts in states that did not change their CNA training requirements. Our baseline estimating equation is as follows:

$$Y_{it} = \beta_1 Training_{st} + \beta_2 X_{it} + \delta_i + \lambda_t + \epsilon_{it}, \quad (1)$$

where *Y* represents the outcomes of interest – log of staffing hours per resident per day (*HPRD*) for nurse assistants, LPN and RN, and the log odds ratio of five quality measures – for nursing home *i* in year *t*.

The treatment variable (*Training*) captures the training requirements for CNA at state *s* in year *t*. Since there are only two states that changed requirements of clinical hours, we focus on two training variables: (1) the required total initial training hours (*Training*); (2) the ratio of clinical to total training hours, represented as $Ratio_{st} = Clinical\ Training\ Hour_{st}/Total\ Training\ Hour_{st}$. When estimating the effect of the clinical-total ratio, we also control for the state requirement of total training hours (*Training*) in the model.

X is a vector of nursing home characteristics such as total number of beds, number of admissions per bed, occupancy rate, ownership status (for-profit vs. nonprofit), hospital-based indicator, percentage of Medicaid patients, percentage of Medicare patients, indicator for any special care unit (SCU) for Alzheimer's patients, indicator for any other special care unit, and percentage of restrained patients (proxy for resident health status). Table 1.2 shows the summary statistics for those control variables as well as the outcome variables.

We include nursing home fixed effects (δ_i) and year fixed effects (λ_t) to control for time-invariant unobservables at the facility level and common shocks that affect all the nursing homes in each year. The error term is represented as ϵ_{it}. We cluster standard errors at the state level to account for within-state correlation in the outcomes.

We also analyze the dynamics in the effect of training requirements by adding the lag of the treatment variable (*Training*) to equation (1). The regression for estimating the dynamics is as follows:

$$Y_{it} = \sum_{j=0,1,2} \gamma_j Training_{st-j} + \gamma_3 X_{it} + \delta_i + \lambda_t + \epsilon_{it}. \tag{2}$$

1.5 Results

Table 1.3 reports the results for Equation (1) that identify the effect of state CNA training requirements on nursing home staffing hours, measured by the log of hours per resident per day separately for nurse aides (NAs), registered nurses (RNs) and licensed practical nurses (LPNs). We study the effects of two treatment variables: state required total training hours and the ratio of state required clinical training hours divided by state required total training hours (with control for total training hours). We find that an increase in total training hours is not associated with any change in staffing level despite a small decrease in NA staffing and total staffing (see Panel A of Table 1.3). However, an increase in the ratio of clinical to total CNA training hours leads to an increase in the staffing of NA, a decrease in LPN staffing, and no change in RN or total staffing levels (see Panel B of Table 1.3). In particular, a one percentage point increase in clinical-total training ratio is associated with a 0.33 percent increase in nurse assistant hours per resident per day (PRD) and a 0.44 percent decrease in LPN hours PRD. We do not find any changes in RN or total staffing hours following changes in the clinical-total training ratio.

We report the results on quality of care in Table 1.4, where outcome variables are measured as the log odds ratio of percent of residents. Similar to the results on staffing, we find no statistically significant change in quality associated with total CNA training hours. However, the point estimates of the clinical-total ratio are negative across all quality measures, suggesting improvement in the quality of care. In particular, an increase in clinical training hours as a proportion of total training hours is associated with a statistically significant decrease in the odds ratio of nursing homes having residents with *Pain* or *Catheter*. The estimates on active daily activity (*ADL*) and *Weight Loss* are not statistically significant but are also negative in sign, suggesting improvement in these quality measures.

We further examine the dynamics of the training requirement's effect on staffing and quality of care. We focus on the clinical to total training hour ratio and test how persistent its effect is by including up to two lags of the ratio variable in the main model. Table 1.5 reports the dynamic effects on nurse/aide staffing. The results suggest that the increase in nurse aide hours takes place immediately after the change in clinical to total ratio; the effect is robust to the inclusion of either one or two lags. However, the effect on total staffing becomes positive and significant after the inclusion of lags, which is predominantly driven by the increase in nurse assistant staffing hours. Table 1.6 reports the results on quality of care. The effects of clinical-total ratio on *Pain* and *Catheter* are consistently negative with the inclusion of the lags. The effects on *ADL* now become statistically significant. Furthermore, the effects on *Catheter* and *WeightLoss* persist two years after the change in the clinical-total training ratio.

1.6 Discussion

Our main results show that an increase in the ratio of clinical to total training hours is associated with consistent decreases in the odds ratio of nursing home residents with ADL, Pain, Catheter and/or Weight Loss, suggesting an improvement in nursing home quality of care from use of higher levels of clinical training for CNAs. Our results on staffing hours suggest that the increase in the ratio of clinical to total training leads to an increase in nurse assistant staffing but a decrease in LPN staffing. This indicates that nursing homes substitute nurse assistants with higher clinical care experience for LPNs, whose costs are higher than those of nurse assistants. The reduced-form results on staffing and quality imply that the improvement in quality of care in nursing homes is likely a result of the improvement in nursing assistant staffing and/or nurse assistant training. At the same time, we do not find any effect of increases in total CNA training hours on either nurse/aide staffing hours or quality of care. Together with the results on the clinical-total ratio, it suggests that clinical training hour has a stronger impact than total training hour on nursing home outcomes.

We further examine heterogeneity in our results by nursing home type. We re-estimate equation (1) after stratifying the sample by two facility characteristics: facility size and for-profit status (as opposed to nonprofit). For facility size, we use total bed size to classify the nursing homes into two groups: small nursing homes are those with less than or equal to 60 beds and large nursing homes are those with more than 60 beds. Under federal staffing regulations, all nursing homes must have one RN who is a full-time director of nurses; for nursing homes with fewer than 60 residents, the director of nursing may also be the charge nurse (Harrington, 2010). Therefore, the cutoff of 60 beds may potentially influence RN staffing and therefore affect other nurse/aide staffing as well.

The results for the sub-sample analyses are shown in Table 1.7. In general, large nursing homes and non-profit nursing homes are more responsive than small or nonprofit nursing homes to changes in the clinical-total training ratio for NA staffing hours. The effects for small nursing homes are not statistically significant for any of the outcomes except for LPN hour (see Column 2 of Table 1.7). But the effects for large nursing homes are consistent with those for the full sample – increasing the clinical-total ratio leads to an increase in nursing assistant staffing and a decrease in LPN staffing as well as an improvement in the quality of care (see Column 1 of Table 7). We also find evidence of improvements in quality of care among both for-profit and nonprofit nursing homes (see Columns 3 and 4 of Table 7). Interestingly, the reduction in LPN staffing is more pronounced in nonprofit nursing homes than in for-profit nursing homes. Also, among for-profit nursing homes, nursing assistant staffing hour is not affected by the rise in the clinical-total ratio.

Taken together, these results suggest that the effect of CNA training on nursing staffing is predominantly driven by large nursing homes and nonprofit nursing homes. Furthermore, they tend to substitute nursing assistants for LPNs, while small nursing homes and for-profit nursing homes reduced LPN staffing without a change in NA staffing. With these changes in staffing, there is improvement in quality of care for almost all measures.

1.7 Conclusion

The training of certified nurse assistants has a strong influence on staffing and direct care in nursing homes. In particular, we find that an increase in the ratio of clinical to total CNA training hours is associated with an increase in nursing aide staffing hours and a decrease in the staffing of registered nurses. Increasing the ratio of clinical training hours also improves quality of care for all the four quality measures – ADL need, weight loss, pain, and catheter use. These effects are more pronounced for large and nonprofit nursing homes. The overall staffing level is slightly increased with a higher clinical training ratio, but the staffing of registered nurses is not affected.

Our findings also highlight differential effects between total training hours and the ratio of clinical hours. While our results of the clinical-total ratio of training hours are similar to those from Trinkoff et al. (2017), we find differential effects of total training hours. Importantly, we find that neither staffing nor quality of care is affected by total training hours. These findings suggest that, compared to total training hours, clinical experience is a more important factor for staffing decisions and quality of care in nursing homes. Considering the complexity of pain identification in long-term care facilities, in-clinic hours in CNA training programs could be important in addressing these problems in direct care.

Our study is subject to several limitations. First, this study estimates reduced-form equations separately for staffing and quality; therefore, the causal relationship between staffing and quality of care cannot be established directly. Second, our data on staffing only contain staffing type and staffing hours, not the number of nurses employed. It would be interesting to know whether nursing homes increased the staffing of nursing assistants by hiring more CNAs or by allowing the existing nursing aides to do more direct-care work. Finally, quality measures are occasionally missing for many nursing homes, leading to an unbalanced sample for quality analysis.

Our results have strong policy implications. We find that nursing homes respond to an increase in required CNA training hours by switching from high-cost RNs to low-cost CNAs with improved training experience. In addition, CNA training is an important factor for quality of care received in nursing homes. The proportion of experience that is clinical experience, in particular, has a strong impact on quality measures related to direct care workers. Thus, it is important to consider including additional clinical hours in current nursing assistant training programs. It is worth noting, however, more and more CNAs are trained in venues outside nursing homes and are pay for their own training (Tyler et al., 2010). Future research should examine whether higher training cost incurred from extended clinical hours would impediment CNA employment.

Table 1.1 Changes in CNA total training hours and clinical training hours during the period 2000-2016

Change in <u>Total</u> Training hours	States	Change in <u>Clinical</u> Training hours	States
<u>Control Group</u>			
Remained 75	AL CO IA KY MA MI MN MS MT NC ND NE NM NV OH OK SD TN VT WY	Remained 16	AL AR CO KY MA MI MN MS NC ND NE NM NV OH OK SD UT WY
Remained 75+	AK AZ CA CT DC DE FL GA HI ID IL IN KS LA MD MO NH NJ NY OR RI WV	Remained 16+	AK AZ CA CT DC DE FL GA HI IA ID IL IN KS LA MD MO MT NH NJ NY OR PA RI SD TN TX VA VT WV
<u>Treated Group</u>			
From 150 to 180	ME (2010)	From 50 to 70	ME (2010)
From 75 to 120	WI (2009)	From 16 to 32	WI (2009)
From 75 to 100	TX (2014)		
From 75 to 90	AR (2009)		
From 75 to 80	PA (2007)		
From 80 to 100	SC (2014) UT (2015)		

Notes: Year in parentheses is the year in which the state implemented the change in training hours as listed at left. Data are collected by the author from state statutes and regulations, cross-referenced with online resources: Office of Evaluation and Inspections (2002), Iowa CareGivers Association (2004), Hernandez-Medina et al. (2006), and Paraprofessional Healthcare Institute (2019).

Table 1.2 Summary statistics

Variable	Definition	N	Mean	Std.Dev.	Min	Max
Training Requirements						
Training	State required total CNA training hours	225,720	97.2	28.4	75	180
Ratio	Clinical to total training hour ratio	225,720	0.37	0.16	0.16	0.71
Staffing (in hours per resident per day)						
NA	Certified nurse aides, aides in training, and medication aides	225,720	2.24	0.76	0.002	13.78
LPN	Registered nurses and director of nurses	225,720	0.784	0.380	0.001	6.86
RN	Licensed practical nurses	225,720	0.395	0.393	0.001	5.99
Total	NA+RN+LPN	225,720	3.42	1.13	0.037	23.77
Quality Measures						
ADL	Percentage with increased need for daily activities	145,640	15.4	8.2	0	93
Pain	Percentage with self-reported moderate to severe pain	132,661	19.5	12.7	0	100
Catheter	Percentage with a catheter inserted	149,613	4.36	3.52	0	94
WeightLoss	Percentage lost too much weight	149,514	7.62	4.53	0	50
Facility Controls						
Beds	Total number of beds	225,720	111.2	63.7	3	1389
Adm/bed	Number of admissions per bed	225,332	1.83	2.43	0	49.9
Occpct	Percent of occupancy rate	225,578	84.6	13.5	3.92	100
For-profit	1=for profit, 0=nonprofit	225,676	0.69	0.46	0	1
Hospital-based	1=hospital based, 0 otherwise	225,720	0.053	0.224	0	1
Medicaid	Percent of Medicaid patients	225,720	62.7	21.1	0	100
Medicare	Percent of Medicare patients	225,720	13.2	13.2	0	100
Alzheimer	Indicator for any Alzheimer's SCU	225,720	0.18	0.39	0	1
AnyUnit	Indicator for any other SCU	225,720	0.21	0.40	0	1
Restrain	Percent of restrained patients	225,720	5.23	8.40	0	100

Note: The sample includes a total of 13,565 nursing homes in the main analysis from NHC 2000 to 2016. The quality measures are only available for 2005-2016.

Table 1.3 Effects of CNA training requirements on nursing home staffing

	(1) NA	(2) LPN	(3) RN	(4) Total
Panel A.				
Total Training Hour	-0.00106*	-0.000128	-0.000784	-0.000743*
	(0.000624)	(0.000688)	(0.000660)	(0.000402)
Panel B.				
Clinical-Total Ratio	0.334*	-0.440**	-0.283	0.171
	(0.193)	(0.169)	(0.243)	(0.132)
Observations	225,147	225,147	225,147	225,147

Note: Standard errors in parentheses are clustered by state. Level of significance: ***<0.01, **<0.05, *<0.1. Outcome variables are measured as log of hours per resident per day for nurse assistants, registered nurses, licensed practice nurses, and total. All models include facility fixed effects and year fixed effects.

Table 1.4 Effects of CNA training requirements on quality of care

	(1) ADL	(2) Pain	(3) Catheter	(4) WeightLoss
Panel A.				
Total Training Hour	0.000560	0.00559	0.00148	0.00273
	(0.00187)	(0.00573)	(0.00281)	(0.00167)
Panel B.				
Clinical-Total Ratio	-1.079	-4.133***	-1.585***	-0.637
	(0.718)	(0.612)	(0.575)	(0.442)
Observations	145,254	132,328	149,220	149,124

Note: Standard errors in parentheses are clustered by state. Level of significance: ***<0.01, **<0.05, *<0.1. Outcome variables are measured as the log of odds ratio for the percentage of residents. All models include facility fixed effects and year fixed effects.

Table 1.5 Dynamic effects of CNA training requirements on nursing home staffing

	NA		LPN		RN		Total	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ratio	0.293**	0.291**	-0.286	-0.348	-0.286	-0.332	0.187**	0.173**
	(0.118)	(0.120)	(0.241)	(0.237)	(0.309)	(0.289)	(0.0784)	(0.0772)
1st lag of Ratio	0.0481	0.200	-0.279	-0.157	0.0135	-0.300	-0.0320	0.0659
	(0.263)	(0.177)	(0.258)	(0.107)	(0.613)	(0.205)	(0.165)	(0.0996)
2nd lag of Ratio		-0.244		-0.166		0.548		-0.149
		(0.236)		(0.296)		(0.801)		(0.182)
Obs.	209,379	194,319	209,379	194,319	209,379	194,319	209,379	194,319

Note: Standard errors in parentheses are clustered by state. Level of significance: ***<0.01, **<0.05, *<0.1. Outcome variables are measured as log of hours per resident per day for nurse assistants, registered nurses, licensed practice nurses, and total. All models include facility fixed effects and year fixed effects.

Table 1.6 Dynamic effects of CNA training requirements on quality of care

	ADL		Pain		Catheter		WeightLoss	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ratio	-1.78***	-1.79***	-3.02**	-2.73**	-1.39**	-1.34**	-0.347	-0.202
	(0.49)	(0.56)	(1.36)	(1.28)	(0.59)	(0.58)	(0.352)	(0.339)
1st lag of Ratio	1.010	0.319	-1.437	0.445	-0.241	0.552	-0.330	0.496
	(1.252)	(0.278)	(1.565)	(0.487)	(0.605)	(0.485)	(0.423)	(0.534)
2nd lag of Ratio		1.224		-3.172		-1.30**		-1.33***
		(1.833)		(1.958)		(0.62)		(0.46)
Obs.	143,776	142,537	130,948	129,831	147,658	146,345	147,560	146,251

Note: Standard errors in parentheses are clustered by state. Level of significance: ***<0.01, **<0.05, *<0.1. Outcome variables are measured as the log of odds ratio for the percentage of residents. All models include facility fixed effects and year fixed effects.

Table 1.7 Effects of clinical-total ratio on nursing home staffing and quality by facility type

	Facility size		Facility type	
	Large	Small	For-profit	Nonprofit
Staffing				
NA	0.384*	-0.0303	0.180	0.566***
	(0.221)	(0.258)	(0.204)	(0.202)
LPN	-0.358*	-1.294***	-0.430**	-0.734***
	(0.203)	(0.360)	(0.204)	(0.202)
RN	-0.358	0.730	-0.441**	0.527*
	(0.254)	(0.485)	(0.181)	(0.314)
Total	0.207	-0.104	0.0649	0.333**
	(0.147)	(0.194)	(0.134)	(0.141)
Quality				
ADL	-1.097	-0.695	-1.128**	-0.0830
	(0.706)	(1.731)	(0.473)	(2.014)
Pain	-4.326***	-0.615	-4.609***	-2.564***
	(0.599)	(0.861)	(0.681)	(0.636)
Catheter	-1.542***	-0.254	-1.809***	-1.170
	(0.562)	(1.077)	(0.611)	(1.271)
WeightLoss	-0.833*	-0.116	-0.598*	-0.966
	(0.425)	(0.891)	(0.310)	(1.429)

Notes: Standard errors in parentheses are clustered by state. Level of significance: ***<0.01, **<0.05, *<0.1. Large size is defined by nursing homes with greater than 60 beds; small size is defined by nursing homes with less than or equal to 60 beds. Staffing variables are measured as log of hours per resident per day. Quality variables are measured as the log of odds ratio for the percentage of residents. All models include facility fixed effects and year fixed effects.

Chapter 2 The Effects of Dental Hygienist Autonomy on Dental Care Utilization

2.1 Introduction

Occupational licensure has grown dramatically over time as employment in the U.S. has shifted from manufacturing to service industries (Kleiner and Krueger 2013). One large component of the service sector, the health care industry, has extensively licensed its occupations. Profession-specific scope of practice (SOP) laws are an important segment of occupational regulations that articulate requirements and govern practice authorities for health care providers. They are especially important for nurse practitioners, dental hygienists, and certified nurse midwives because these professionals are generally required to work under the supervision of a more highly trained health professional (e.g., a physician or dentist). Changes in the autonomy level of these professionals significantly affect the boundary of services and settings in which the professional can render services. Some have argued that scope of practice laws in many U.S. states prevent dental hygienists from fulfilling their potential to improve access to oral healthcare (Manski et al. 2015; Reinders et al. 2017). Since 1988, new legislation has gradually been implemented in many states that enables dental hygienists to perform more tasks without the supervision of a dentist. At the time of this writing, Colorado is the only state that places no restriction on independent dental hygiene practices. Advocates of these laws believe that this increase in autonomy will potentially enhance access to dental health care and improve the efficient delivery of services in underserved areas. Also, expanding the scope of practice for dental hygienists is one way to help dental providers meet the rising demand for dental services due to, for example, provisions of the Affordable Care Act (Meyerhoefer et al. 2016). However, little is known about the effect of dental hygienist scope of practice on the utilization of dental care.

Previous research on state-specific SOP laws for dental hygienists primarily focuses on labor and product market effects. For example, Wanchek (2010) and Kleiner and Park (2010) find that stringent practice regulations on dental hygienists result in lower wages and slower employment growth; Wing and Marier (2014) find that expanding the SOP reduces the prices of basic dental services. However, there are no studies that we are currently aware of that determine the impact of scope of practice laws for dental hygienists on both dental care use and costs. We seek to fill this gap in the literature.

There are many channels through which scope of practice laws for dental hygienists could influence the utilization of dental care. Greater autonomy for hygienists may increase dental care utilization if more hygienists are able to practice where there are fewer dentists and therefore reduce pecuniary and non-pecuniary travel costs and wait times for consumers. Also, expanding hygienists' SOP can enable them to administer basic teeth cleanings and examinations without supervision, thereby freeing up dentists to perform more complex procedures. On the other hand, if consumers view hygienists' clinical competence as lower than that of dentists, they may reduce their consumption of dental care in the regions that allow hygienists to perform more procedures. Therefore, the overall effect of hygienist regulations on dental care utilization must be determined empirically.

We exploit variation within states over time in SOP laws in order to determine their effect on dental care use and expenditure. We collected detailed information on dental hygienist SOP regulations from individual state statutes from 2001 to 2014 and merged these to data on dental care utilization from the Medical Expenditure Panel Survey (MEP. Using a difference-in-difference approach, we find evidence that increasing the autonomy level of dental hygienists increases the use of preventive dental services and leads to a small increase in dental care costs. Increases in dental care visits are more pronounced in areas with a shortage of dental care providers.

2.2 Background

The training program for dental hygienists was first established in 1913, and as of 2016 there were a total of 336 baccalaureate and masters-level programs to train dental hygienists (Mertz 2016). Dental hygienists are required to be licensed in all states, which have different requirements and different scope of practice regulations. Typically, dental hygienists are allowed to perform basic dental services with or without a dentist's oversight. Such services are usually considered preventive dental care and primarily include cleaning teeth, screening patients for oral health status, applying sealants or fluorides, and taking dental X-rays. In some states with expanded scope of practice, hygienists are allowed to place amalgam restorations and administer local anesthesia.[6]

2.2.1 Literature Review

Similar to other health fields, in the dental field there is intra-professional friction between dentists and dental hygienists over scope-of-practice rules. Complicating the push for expanded SOP by hygienists is the fact that dental hygienists are regulated by state dental boards made up primarily of dentists (Koppelman et al. 2016).

[6] See https://www.bls.gov/ooh/healthcare/dental-hygienists.htm for more details on dental hygienists' duties.

An important issue for policy makers is whether the expansion of provider SOP may improve the efficiency of healthcare delivery. The economic theory of licensure regulations predicts that more stringent regulations may reduce wages and employment of the regulated professional (Kleiner 2016). Empirically, these predictions are borne out in research on the SOP of nurse practitioners (Kleiner et al. 2016; Perry 2009; Stange 2014), physician assistants (Perry 2009; Stange 2014; Timmons 2017), and chiropractors (Timmons et al. 2016). In the case of dental hygienists, Wanchek (2010) and Kleiner and Park (2010) also find that stringent practice regulations for hygienists result in lower wages and slower employment growth, but they do not examine dental care delivery.

In theory, occupational regulations for health care providers are expected to raise the price and reduce the demand for the health care services. However, there are very few studies that examine how these regulations affect the health care system. Kleiner et al. (2016) find that stronger regulation of nurse practitioners raises the price of well-child visits. Markowitz et al. (2017) finds that states with no barriers for certified midwives have a higher probability of midwifery attended births and small improvements in infant birth weight. Using the MEPS, Traczynski and Udalova (2018) finds that nurse practitioner independence increases the probabilities of a routine checkup and being able to get an appointment when wanted. All of the three aforementioned studies use a difference-in-differences framework to identify the effect of regulatory changes.

Four studies on SOP laws are specific to dental care. Kleiner and Kudrle (2000) collected dental legislative information for the period from 1960 to 1994 and compared the oral health outcomes of Air Force personnel over time to show that stricter scope of practice regulations raise the price of dental services and earnings of dentists, but do not lead to better oral health.

Wing and Marier (2014) used the 2005-2007 FAIR claims data to estimate the prices of seven basic services that hygienists are allowed to perform. They found that regulations that limit the authority of dental hygienists to provide services increase the prices of those services by approximately 12 percent but do not affect dental care utilization.

Wing et al. (2005) developed an index to assess practice environment of dental hygiene across all the US states in the year 2001. They used this index along with the 2002 Behavioral Risk Factor Surveillance System (BRFSS) survey to make cross-state comparisons of dental care utilization. They report that states with expanded scope of practice for dental hygienists (or higher practice environment scores) exhibit larger utilization of dental care. Langelier et al. (2016) updated the index developed by Wing et al. (2005) to 2014 and merged the 2001 and 2014 indices to the 2001 and 2012 BRFSS, respectively to show that greater autonomy for dental hygienists is associated with a lower probability of having had any teeth removed because of decay or disease. However, one limitation of both studies is that they only estimate cross-sectional associations.

All of these except Kleiner and Kudrle (2000) use the BRFSS. The outcome variable in the BRFSS that measures dental care utilization is constructed from a survey question that asks whether the respondent has ever had a teeth cleaning by a dentist or dental hygienist in the previous year, which only allows analysis at the extensive margin of preventive dental care. In contrast, we use the Medical Expenditure Panel Survey (MEPS), which contains a large set of dental care utilization measures. In particular, we are able to construct measures of preventive care visits, visits for dental treatment, hygienist visits, and dentist visits. Furthermore, exploiting the panel dimension of the MEPS allows us to mitigate omitted variable bias from the presence of unmeasured characteristics of states and patient populations that are correlated with practice environments.

2.2.2 Dental Hygiene Professional Practice

Like several previous studies we make use of the Dental Hygiene Professional Practice Index (DHPPI). In order to quantify the SOP of dental hygienists for all states, the Center for Health Workforce Studies first created the DHPPI for the year 2001 and updated the index for the year 2014 using the same set of components (Langelier et al., 2016). The index is normalized to range between 0-100 with higher scores reflecting a more autonomous practice environment for dental hygienists. Each of component of the DHPPI is assigned a weighted score to reflect its relative impact on the ability of the dental hygienists to provide services. The four broad components in DHPPI are (1) *Legal and regulatory environment*,[7] (2) *Supervision levels in different practice settings,* (3) *Tasks permitted under varying levels of supervision*,[8] and (4) *Reimbursement*.

We extend the DHPPI to the 2002-2013 period by gathering information on each component of the DHPPI from state statutes and regulations, cross-referenced by advocacy files at the American Dental Hygienists' Association. Rather than using the index itself to measure changes in dental hygienist SOP, we use the changes in the supervision component of the DHPPI to identify the effect of dental hygienist SOP laws in our analysis. Variables in the *Reimbursement* component, indicating whether direct reimbursement to hygienists is available

[7] The variables in *Legal and regulatory environment* capture governance of the profession through the state regulatory board of dental hygiene or a dental hygiene committee empowered by a dental board with a mandate to regulate the profession, licensure by credential/endorsement with no new clinical exam required, scope of practice defined in law or regulations, and restriction to the patient of record of the primary employing dentist.

[8] Tasks evaluated in this section include prophylaxis, sealants, fluorides, X-rays, hygiene screening and assessment, as well as expanded functions such as placing amalgam restorations, administration of local anesthesia, and administration of nitrous oxide.

from Medicaid and whether direct payment is allowed from other third-party insurers or patients, are used as control variables for dental hygienists' financial incentives in our model.

Following the supervision requirements defined in the DHPPI, we categorize dental hygienist autonomy into four levels, ranging from direct supervision (level one), general supervision (level two), collaborative supervision (level three), to unsupervised practice (level four). The definitions of the four autonomy levels are given below. As indicated, the higher the autonomy level, the lower the supervision requirement:

Level 1, Direct Supervision: The dental hygienist practice is mandated under the direct supervision of a dentist, with supervision requirements specified.

Level 2, General Supervision: A dental hygienist practicing under the general supervision of a licensed dentist shall have a written agreement with the supervising dentist that clearly sets forth the terms and conditions under which the dental hygienist may practice. A dentist needs to authorize prior to services but needs not be present.

Level 3, Collaborative Agreement: The hygienist may practice without supervision, pursuant to a collaborative agreement with a dentist. A dental hygienist who has entered into a collaborative agreement may perform dental hygiene services on children, senior citizens age 65 and older, and persons with developmental disabilities in long-term care facilities, free clinics, hospitals, head start programs, residences of homebound patients, local health units, schools, community health centers, and state and county correctional institutions. The dental hygienist must have a written agreement with no more than one dentist.

Level 4, Unsupervised Practice: There is no requirement that a dentist must authorize or supervise most dental hygiene services. The dental hygienist may also own a dental hygiene practice.

Dental supervision requirements may differ across different practice settings, even though the same level of autonomy applies to most practice settings within a state. These practice settings include private dental offices, long-term care facilities, schools, public health agencies, correctional facilities, and similar institutional facilities. In our analysis, we use changes in the highest autonomy level allowed among all the evaluated settings as our treatment variable. The reason that we focus on the variation in autonomy level instead of the variation in permitted tasks is that each state has specific hygiene duties in the dental statutes that dictate the level of supervision requirement. Thus, in practice, it is still the permitted autonomy level that dominates in the performance of the dental service.

Figure 2.1 shows the changes in the highest autonomy level among all practice settings across states between 2001 and 2014. During the period 2001-2014, the hygienist autonomy level increased in 19 states and remained unchanged in all other states. Note that as of 2014 there were still 25 states that restrict dental hygienist autonomy to level 2 (general supervision) or lower.

2.3 Empirical Model and Data

2.3.1 Empirical Model

To assess the impact of regulatory changes on the outcomes of interest, we use a difference-in-differences approach in which we compare individuals in states that experienced a change in their SOP regulations for hygienists during the sample period to individuals in states that did not change their regulations. Our baseline estimating equation is as follows:

$$Y_{it} = \beta_1 H\_Autonomy_{st} + \beta_2 X_{it} + \beta_3 Z_{st} + \delta_s + \lambda_t + \epsilon_{st}, \qquad (1)$$

where Y_{it} represents the outcome of interest—dental care visits and expenditure—for individual *i* in state *s* at year *t*; $H\_Autonomy_{st}$ is the treatment variable that indicates high autonomy level (level 3 or level 4) pertaining to hygienists in state *s* at year *t*. X_{it} is a vector of individual-level control variables including age, race and ethnicity, marital status, region indicators (Northeast, South, Midwest, and West), urban residence, years of education completed before entering the survey, number of children under 5 or under 18 in the household, log of income earned by other family members (normalized by household size), self-reported physical and mental health status, and a disability indicator. Z_{st} is a vector of county-level factors obtained from the Area Resource File that may also influence the health care market, including real income per capita, the unemployment rate, the poverty rate, and the percentage of the population with college degree or higher. δ_s and λ_t are state fixed effects and year fixed effects, respectively. We estimate the models with the sampling weights and robust standard errors clustered at the state level (Cameron and Miller 2015).

Due to the fact that the distribution of dental care visits and expenditures is right-skewed and has a mass point at zero, we estimate a two-part model for each type of visit and expenditure. The first part of the model captures the extensive margin using a dummy variable indicating whether an individual had at least one visit, and the second part of the model captures the intensive margin using the log of the number of visits or the log of the dollar amount of expenditure. The first part is specified as a logit model:

$$\Pr(Y_{ist} = 1) = \Lambda(\beta_1 H\_Autonomy_{st} + \beta_2 X_{it} + \beta_3 Z_{ct} + \delta_s + \lambda_t), \qquad (2)$$

The second part is specified as a log-linear regression model and is estimated for the sample with non-zero visits or expenditures:

$$\ln(Y_{ist}) = \gamma_1 H\_Autonomy_{st} + \gamma_2 X_{it} + \gamma_3 Z_{ct} + \delta_s + \lambda_t + \epsilon_{st}, \#(3)$$

The full two-part model is calculated using the predicted probability from the first part and the estimated conditional mean from the second part. In order to transform regressors from the log value to the raw value, we use Duan's smearing transformation (Duan et al. 1984).[9]

2.3.2 Outcome Variables

Our main data source is the 2001-2014 Medical Expenditure Panel Survey (MEPS), which is a nationally representative survey of the non-institutionalized population in the U.S. that contains detailed information on health and dental care use and costs, as well as a large number of individual socio-economic characteristics. We use observations in the Full Year Consolidated Data Files of the MEPS as a repeated cross-section to create a panel of states over time. The total number of observations in our sample is 489,278.

The dental event file component of the MEPS is used to construct different measures of dental care visits and expenditures. In addition to the date of the dental visit and all payments by the individual and third parties, the files also contain information for each dental visit on the type(s) of provider the individual sees, the specific health service(s) the individual uses. We use information on the provider types (e.g., general dentist, dental hygienist, or oral surgeon)

[9] To gauge the potential for heteroscedasticity to bias the estimates, we compute the smearing factors across different groups (children vs. adults, having dental insurance vs. having no dental insurance). However, the estimates are the same using the more disaggregated smearing factors as using a constant smearing factor, so we use the latter (Duan et al. 1984; Manning 1998).

identified on an event record to categorize dental visits into visits to only a dentist, visits to only a dental hygienist, and visits to both providers. For the type(s) of dental services or treatments received during the visit, we categorize these services into preventive dental care (examinations, teeth cleanings, X-rays, fluoride applications, and sealant applications) and treatment services for all the other services including but not limited to root canals, fillings, inlays, crowns, gum surgeries, tooth extractions, implants, bridges, dentures, repairs, and whitening. Thus, the variables we construct for dental care visits include total dental visits, preventive care visits, dental treatment visits, general dentist (GD) visits, dental hygienist (DH) visits, and visits to both providers. We also estimate models on specific types of dental services, where we focus on the services analyzed in the DHPPI: prophylaxis, applications of sealant, fluoride treatments, X-rays, hygiene screening and assessment, as well as expanded function of placing amalgam restorations.[10] In addition to total dental care expenditure, we construct variables for out-of-pocket dental expenditure and third-party payments for dental care.

We also use the MEPS to measure individual perceptions of access to dental care. In the MEPS, respondents are asked “Was {the person} unable to get (delayed in getting) necessary dental care?” and parents are asked about children aged 2-17 “Has a doctor or other health provider ever given you {or the person} advice about {him/her} having regular dental check-ups?” We use individual responses to these questions to construct three binary indicators of access to dental care: (1) whether the person was unable to receive necessary dental care, (2) whether the person was delayed in receiving necessary dental care, and (3) whether doctors ever advised the child to have a regular dental checkup. We also include the individual's self-reported

[10] Since the service of amalgam restorations is not in the list of dental services in the MEPS, we instead use the service type of “dental fillings”.

frequency of dental checkups; we code the response as 1 if the respondent reported the frequency as “less than once a year” or more, and 0 otherwise. Table 2.1 shows the summary statistics for all the outcome variables and the control variables.

2.3.3 Scope of Practice Regulations

In order to use the difference-in-difference framework, we must compare individuals in the states with a law change to those in the states without a law change. We categorize the SOP laws for dental hygienists using the regulatory variable measuring the highest level of autonomy allowed among all practice settings. We report the change in highest level of autonomy for each state from 2001-2014 in Table 2.2. Columns 1-3 of Table 2.2 summarize the changes across states while Columns 4-6 specify the states that are used in each specification as either a treatment state or control state. Note that there is a great deal of heterogeneity in the change of autonomy across states, but the vast majority of variation comes from the changes from lower levels, level 1 (direct supervision) and level 2 (general supervision), to higher levels, level 3 (collaborative agreement) and level 4 (unsupervised practice). Thus, we include in the treatment group individuals in the states that changed from lower levels to higher levels of autonomy, while the control group is comprised of individuals in the states that kept autonomy at lower levels. In order to test the sensitivity of the estimates to different definitions of treatment and control, we use three samples with different treatment/control groups for the difference-in-difference analysis: (A) treated—states where the dental hygienist (DH) autonomy increased from lower levels to higher levels, and control—states where the autonomy level remained at lower levels; (B) treated—states where the DH autonomy increased from level 2 to higher levels, and control—states with DH autonomy level remained 2; (C) treated—states where the DH autonomy level increased from 2 to 3, control—states where the DH autonomy level remained 2.

Notably, over half of the changes in the DH autonomy level come from changes from level 2 to higher levels, especially changes from level 2 to level 3. Our preferred specification is the one where the treatment group is those states where the DH autonomy level changed from 2 to 3 (Panel C). This is because Panel C represents a more homogeneous grouping of states in the treatment and control groups.

2.4 Results

Table 2.3 shows the results of the difference-in-differences models that identify the effect of dental hygienist autonomy on total dental visits using the three different subsamples (Panels A-C, as specified in Table 2.3). For each type of visit, we estimate a two-part model and report the total marginal effect, extensive margin effect and intensive margin effect. Column (1) of Table 2.3 shows that an increase in the DH autonomy level leads to an increase in total dental care use by 0.09 visits, although the effect is only precisely estimated when we consider states that increase their DH autonomy from level 2 to level 3 relative to states that remained at level 2. Column (2) shows that there is no statistically significant change in total dental visits at the extensive margin, while Column (3) of Panel B and Panel C show that changing the autonomy level from 2 to 3 or higher levels results in a significant increase of about 2.6 - 5.0 percent in total dental care visits among those with any dental visit. We also find that these increases in dental care utilization are mostly for preventive dental care services. In particular, Column (4) of Panel C shows that increasing the DH autonomy level from 2 to 3 is associated with a significant increase of 0.04 preventive care visits. Similar to Column (4), Column (6) also shows a significant increase in the number of preventive care visits at the intensive margin in Panel B and Panel C. Note that Columns (7)-(9) contain no significant effects on either the extensive or the intensive margin for dental treatment visits. Overall, our results suggest that higher DH autonomy increases preventive care visits but does not affect visits for dental treatment.

Table 2.4 reports the results of the impact of DH autonomy on total dental expenditure, third-party payments, and self-payments using the same structure as Table 3. As shown in Column (1) of Panel C, increasing the DH autonomy level from 2 to 3 leads to an increase in total expenditure of 23.96 dollars per year. The increase mostly comes from the intensive margin (Column 3). Moreover, this increase in total expenditure is primarily due to higher third-party payments. In particular, columns (4)-(6) of Panel C show a significant increase in third-party payments for dental care at the extensive margin. We find no significant effects on self-payment with the exception of a small reduction of 0.9 percent in the probability of any positive amount of self-payment for states that increased from lower autonomy levels to higher autonomy levels (Column 8 of Panel A and Panel C).

2.5 Discussion

2.5.1 Robustness check

We examine whether pre-existing trends in dental care utilization are causing us to overestimate the effect of dental hygienist autonomy on use and expenditure. To test for the presence of pre-existing trends, we estimate equation (1) after adding three leads of the regulatory dummy. We report the test for the results of the effect of DH autonomy on total dental care visits as well as the intensive margin effect in Table 2.5. We do not find any statistically significant effects in the pre-treatment dummies, which is consistent with the parallel trend assumption of our difference in difference model.

2.5.2 Specific dental services

Increasing the autonomy level allows hygienists to perform more preventive care services such as examinations, teeth cleanings, X-rays and sealant applications, as well as restorative services such as applications of amalgam fillings. Given that our main results suggest that more basic preventive care is consumed after an increase in DH autonomy, we further investigate the effect of DH autonomy on each specific task that hygienists are usually allowed to perform. For each type of visit, we estimate a two-part model of these tasks and report only the total marginal effect (Table 2.6). We find statistically significant increases in many of the specific services. Columns (1)-(3) show an increase of 0.04 total visits in teeth examination, teeth cleaning, and X-rays for states that increased from autonomy level 2 to level 3. Column (4) shows that increasing DH autonomy leads to significant increases in utilization of X-rays and fluoride applications of 0.03 visits in Panel A and Panel B. However, we do not find any statistically significant effects on the application of sealants, which may be due to the small occurrence of sealant visits in our sample. Interestingly, we find a significant reduction in the application of fillings in both Panel A and Panel B (Column 6 of Table 2.6). Filling application is one of the few restorative services that hygienists can perform. However, individuals may receive fewer fillings when hygienists have more autonomy because the greater availability of hygienists leads patients to use more preventive care, which typically decreases the need for restorative treatment.

2.5.3 Access to care by adults and children

Given that measures of dental care utilization and expenditure are responsive to the expansion of hygienist autonomy, it is possible that individuals in states that expanded scope of practice have better access to the dental care system as a result. We re-estimate equation (1) with indicators of several other dental care outcomes as dependent variables and report the results in Table 2.7.

Column (1) examines whether an individual reports any regular dental checkup. Approximately twenty percent of our sample reports never going to a dentist. We find no evidence that increasing DH autonomy affects the probability of reporting any regular dental checkup, which is consistent with our finding of no effect in the main results for dental care utilization at the intensive margin.

Columns (2) and (3) of Table 2.7 examine the effect of higher autonomy on individuals' perception of the accessibility of receiving necessary dental care. The two indicators that we constructed are for "whether the person was unable to receive necessary dental care" and "whether the person was delayed in receiving necessary dental care".[11] Our estimates suggest that more DH autonomy is not significantly associated with a greater accessibility of necessary dental care.

The results are somewhat different when we focus on children and examine whether doctors ever advised the child to have a regular dental checkup. About fifty percent of children in our sample report ever receiving this advice. Column (4) shows that more DH autonomy increases the probability that doctors advise that children have a dental checkup by around 5 percentage points in all the panels. We therefore re-estimate our main specification on dental care utilization when sub-setting the sample to children only. Table 2.8 presents the results. Column (1) shows that the estimated effects of expanding DH autonomy on total dental visits for children are larger than the effects for the full sample. Column (3) shows that the intensive margin effects are also larger and are statistically significant across all treatment/control samples.

[11] The two questions were not asked in 2001.

2.5.4 Labor substitution

One potential consequence of DH autonomy is labor substitution between general dentists (GD) and dental hygienists. Dental hygienists that no longer need to work under the direct supervision of dentists may spend more time on basic dental tasks and reduce the time spent on those tasks by dentists. However, since hours worked by providers during a visit are unknown in our data, we rely on the total number of visits to each type of provider to estimate labor substitution effects. We focus on three types of visits: visits to both DHs and GDs, visits to only GDs and visits to only DHs. Table 2.9 presents the results of the impact of DH autonomy on the three types of visits. The estimates are small and are not statistically significant, and we find no evidence for labor substitution between general dentists and hygienists.

2.5.5 Health professional shortage areas

An important policy question is whether DH independence improves access to care for underserved populations. The Department of Health and Human Services (HHS) defines Health Professional Shortage Areas (HPSA) as geographical areas with an insufficient number of health care providers. We merge the Area Health Files provided by HHS to our data to identify shortage areas for dental care providers, and we re-estimate our model in Table 2.3 by sub-setting each sample to individuals residing in the counties with HPSA populations. The results, which are shown in Table 2.10, are very similar to our main results, but the estimated effects are more robust across different treatment/control samples. In particular, the estimates in Column (3) are positive and significant for all panels, suggesting stronger effects of SOP laws on dental care utilization for underserved populations than for the full population.

2.6 Conclusion

In this paper, we study the effects of occupational regulation in the form of scope of practice regulations for dental hygienists in different practice settings. We investigate changes in the autonomy level of dental hygienists across states over a long time period. Using a difference-in-differences approach, our results show that dental care utilization increases when hygienists have more autonomy; notably these increases in utilization are primarily for preventive care services. The results are robust to the assumption of parallel trends in the difference-in-difference model. We find that allowing hygienists to work more independently from dentists increases the intensity of dental visits but does not necessary bring more people into dental treatment. Consistent with the findings of Wing and Marier (2014), we find no evidence that increasing autonomy levels affect dental care visits at the extensive margin.

In further analyses we find that these increases in the number of dental visits are more pronounced in provider-shortage areas, for specific preventive dental tasks, and for children. We find that utilization of basic services such as teeth examinations, cleanings, X-rays, and fluoride applications all increase by 0.03-0.04 visits when hygienists are granted more autonomy from dentists. Given that the cost-effectiveness of preventive care is higher than dental treatment, our results suggest increasing DH autonomy leads to more efficient dental care delivery. From a policy perspective, the fact that we find increases in utilization in areas with health professional shortages suggests that relaxing the regulatory barriers to supervision requirements could improve access to dental care for those with limited access to providers.

Our study has several limitations that should be noted. First, the MEPS does not contain information on the quality of care received during dental visits. As a result, we are not able to directly examine how scope of practice laws for dental hygienists affect the quality of dental care. In addition, we do not have information on the hours worked by dental providers. Therefore, we are unable to directly estimate labor substitution effects. Future research using information on service hours by providers could shed light on the change in labor supply between dental hygienists and dentists after the expansion of scope of practice for dental hygienists.

Figure 2.1 Dental hygienist autonomy level by state in 2001 and 2014

Figure 2.1a. Dental hygienist autonomy level by state in 2001

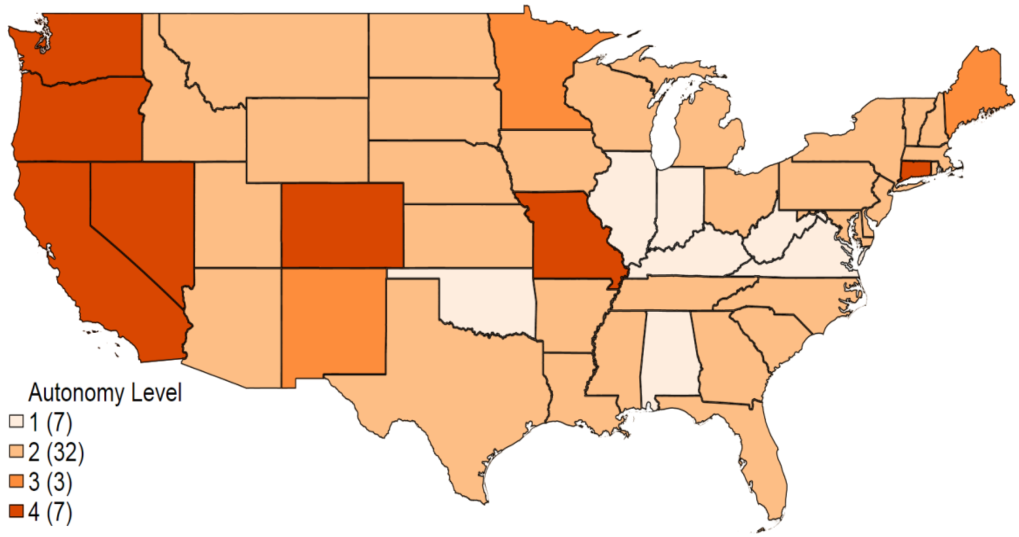
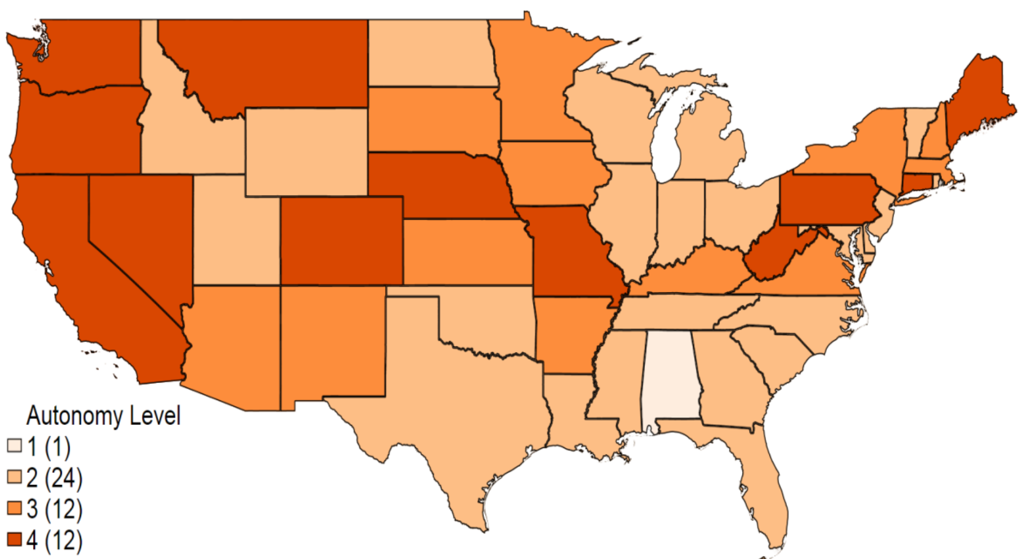


Figure 2.1b. Dental hygienist autonomy level by state in 2014



Notes: Data were collected by authors from state statutes and regulations, and cross-referenced with advocacy files at the American Dental Hygienists' Association.

Table 2.1 Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Total dental visits	0.81	1.60	0	55
At least one dental visit	0.36	0.48	0	1
DH visit	0.23	0.60	0	17
GD visit	0.41	0.99	0	39
Preventive care visit	0.53	0.92	0	38
Dental treatment visit	0.27	0.87	0	50
Log of self-pay dental expenditure	1.07	2.18	-0.64	10.64
Log of TPP dental expenditure	1.47	2.47	-1.33	10.63
Log of total dental expenditure	1.97	2.78	-0.45	10.67
Age	34.53	22.48	0	85
Female	0.52	0.50	0	1
Hispanic	0.28	0.45	0	1
Black	0.19	0.39	0	1
Other race	0.07	0.25	0	1
Married	0.36	0.48	0	1
Family size	3.50	1.81	1	14
Education year	9.44	5.56	0	19
Log of family income per person	9.95	1.70	-1.01	13.59
Self-reporting indicator	0.39	0.49	0	1
Pprivate insurance	0.49	0.50	0	1
Medicare	0.13	0.33	0	1
Medicaid	0.22	0.41	0	1
Dental insurance	0.39	0.49	0	1
Ever employed during survey year	0.48	0.50	0	1
Urban	0.83	0.38	0	1
Self-reported health	0.17	0.37	0	1
Self-reported mental health	0.10	0.29	0	1
Disability indicator	0.05	0.22	0	1
Observations	489,278			

Note: All means are calculated using the MEPS sampling weights.

Table 2.2 Changes in highest autonomy level allowed for DH during period 2001-2014

Change in Autonomy Level	States	Panel A	Panel B	Panel C
Control Group				
Remained 1	AL	X		
Remained 2	DC DE FL GA HI ID IL LA MD MI MS NC ND NJ OH OK RI SC TN TX UT VT WI WY	X	X	X
Remained 3	NM MN			
Remained 4	CA CT CO MO NV OR MA			
Treated Group				
From 1 to 2	IN(10)			
From 1 to 3	KY(11) VA(10)	X		
From 1 to 4	WV(04)	X		
From 2 to 3	AK(09) AZ(03) AR(12) IA(05) KS(04) MA(10) NH(13) NY(14) SD(13)	X	X	X
From 2 to 4	MT(04) NE(08) PA(10)	X	X	
From 3 to 4	ME(09)			

Notes: Year in parentheses is the year that the state implemented the change in autonomy level listed at left. Data were collected by the authors from state statutes and regulations, cross-referenced with advocacy files at American Dental Hygienists' Association.

Table 2.3 Effect of dental hygienist autonomy level on dental care visits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Total Visits			Preventive Care Visits			Treatment Visits	
VARIABLES	Visits	Visit>0	ln(Visits)	Visits	Visit>0	ln(Visits)	Visits	Visit>0	ln(Visits)
Panel A: Treated: DH autonomy level increased from lower levels to higher levels; Control: DH autonomy level remained lower levels									
Autonomy level	0.013	-0.004	0.020	-0.003	-0.004	0.008	-0.017	-0.005	-0.010
	(0.0211)	(0.005)	(0.014)	(0.014)	(0.005)	(0.012)	(0.016)	(0.006)	(0.012)
Observations	308,449	308,449	113,070	308,449	308,449	102,248	308,449	308,449	45,766
Panel B: Treated: DH autonomy level increased from 2 to higher levels; Control: DH autonomy level remained 2									
Autonomy level	0.030	-0.0004	0.026*	0.016	0.0004	0.019***	-0.008	-0.002	-0.007
	(0.024)	(0.0064)	(0.013)	(0.014)	(0.0061)	(0.007)	(0.014)	(0.005)	(0.011)
Observations	280,367	280,367	102,847	280,367	280,367	93,018	280,367	280,367	41,477
Panel C: Treated: DH autonomy level increased from 2 to 3; Control: DH autonomy level remained 2									
Autonomy level	0.089***	-0.004	0.050***	0.040***	-0.004	0.022**	0.020	-0.005	0.012
	(0.024)	(0.005)	(0.016)	(0.014)	(0.005)	(0.009)	(0.013)	(0.006)	(0.013)
Observations	261,884	261,884	94,940	261,884	261,884	85,824	261,884	261,884	38,268

Notes: level of significance: ***<0.01, **<0.05, *<0.1. For each type of visit, we estimate a two-part model where the first part is a logit regression using an indicator of any visit and the second part is a log-linear regression. Columns (1), (4) and (7) report the total marginal effect from the two-part model, Columns (2), (5) and (8) report the marginal effect from the logit regression, and Columns (3), (6) and (9) report the marginal effect (i.e. coefficient) from the second part. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Table 2.4 Effect of dental hygienist autonomy level on dental care expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Total Expenditure			Third-Party Payment			Self Payment	
VARIABLES	Exp	Expt>0	ln(Exp)	Exp	Exp>0	ln(Exp)	Exp	Exp>0	ln(Exp)
Panel A: Treated: DH autonomy level increased from lower levels to higher levels; Control: DH autonomy level remained lower levels									
Autonomy level	1.856	-0.003	0.017	5.14	0.002	0.040	-3.285	-0.009*	0.003
	(7.751)	(0.005)	(0.036)	(4.81)	(0.006)	(0.040)	(5.033)	(0.006)	(0.043)
Observations	308,449	308,449	110,007	308,449	308,449	84,830	308,449	308,449	68,310
Panel B: Treated: DH autonomy level increased from 2 to higher levels; Control: DH autonomy level remained 2									
Autonomy level	4.127	-0.0003	0.021	4.30	0.004	0.029	-1.720	-0.004	-0.002
	(9.790)	(0.0072)	(0.045)	(5.7544)	(0.009)	(0.046)	(5.479)	(0.005)	(0.048)
Observations	280,367	280,367	100,038	280,367	280,367	77,055	280,367	280,367	61,752
Panel C: Treated: DH autonomy level increased from 2 to 3; Control: DH autonomy level remained 2									
Autonomy level	23.96***	-0.003	0.090**	15.75***	0.002	0.088**	6.118	-0.009*	0.059
	(8.52)	(0.005)	(0.040)	(4.94)	(0.006)	(0.039)	(6.111)	(0.006)	(0.052)
Observations	261,884	261,884	92,334	261,884	261,884	71,112	261,884	261,884	57,080

Notes: level of significance: ***<0.01, **<0.05, *<0.1. For each type of expenditure, we estimate a two-part model where the first part is a logit regression using an indicator of any expenditure and the second part is a log-linear regression. Columns (1), (4) and (7) report the total marginal effect from the two-part model, Columns (2), (5) and (8) report the marginal effect from the logit regression, and Columns (3), (6) and (9) report the marginal effect (i.e. coefficient) from the second part. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Table 2.5 Specification test for effect of parallel trend assumption in difference-in-difference model

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A		Panel B		Panel C	
Variables	Total Visits	ln(Visits)	Total Visits	ln(Visits)	Total Visits	ln(Visits)
Autonomy	0.0224	0.0216	0.0216	0.0696	0.0696	-0.0058
	(0.0267)	(0.0371)	(0.0371)	(0.0449)	(0.0449)	(0.0061)
1^{st} lead of Autonomy	-0.0086	0.0039	0.0039	0.0112	0.0112	-0.0011
	(0.0329)	(0.0379)	(0.0379)	(0.0444)	(0.0444)	(0.0134)
2^{nd} lead of Autonomy	0.0064	0.0281	0.0281	0.0496	0.0496	0.0124
	(0.0274)	(0.0301)	(0.0301)	(0.0385)	(0.0385)	(0.0077)
3rd lead of Autonomy	-0.0129	-0.0343	-0.0343	-0.0645	-0.0645	-0.0127
	(0.0451)	(0.0475)	(0.0475)	(0.0600)	(0.0600)	(0.0140)

Notes: level of significance: ***<0.01, **<0.05, *<0.1. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Table 2.6 Effect of dental hygienist autonomy level on specific dental tasks

VARIABLES	(1) Examination	(4) Cleaning	(7) X-ray	(10) Fluoride	(13) Sealant	(16) Fillings
Panel A: Treated: DH autonomy level increased from lower levels to higher levels; Control: DH autonomy level remained lower levels						
Autonomy level	0.001	-0.002	0.004	0.024**	0.003	-0.018**
	-0.015	(0.011)	(0.013)	(0.009)	(0.003)	(0.008)
Observations	308,449	308,449	308,449	308,449	308,449	308,449
Panel B: Treated: DH autonomy level increased from 2 to higher levels; Control: DH autonomy level remained 2						
Autonomy level	0.010	0.007	0.012	0.026**	0.003	-0.017**
	(0.014)	(0.014)	(0.016)	(0.0115)	(0.004)	(0.009)
Observations	280,367	280,367	280,367	280,367	280,367	280,367
Panel C: Treated: DH autonomy level increased from 2 to 3; Control: DH autonomy level remained 2						
Autonomy level	0.037**	0.038***	0.035**	0.012	-0.002	-0.010
	(0.017)	(0.012)	(0.015)	(0.018)	(0.004)	(0.010)
Observations	261,884	261,884	261,884	261,884	261,884	261,884

Notes: level of significance: ***<0.01, **<0.05, *<0.1. For each type of visit, we report the total marginal effect from the two-part model where the first part is a logit regression using an indicator of any visit and the second part is a log-linear regression. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Table 2.7 Effect of dental hygienist autonomy level on access to dental care measures

Variable	(1) Any Dental Checkup	(2) Unable to Access	(3) Delayed Access	(4) Ever Advised Checkup (Children)
Panel A. Treated: DH autonomy level increased from lower levels to higher levels; Control: DH autonomy level remained lower levels				
Autonomy level	-0.0046	0.0014	0.0008	0.0482**
	(0.0070)	(0.0052)	(0.0027)	(0.0188)
	308,449	287,222	287,222	80,807
Panel B. Treated: DH autonomy level increased from 2 to higher levels; Control: DH autonomy level remained 2				
Autonomy level	-0.0029	0.0036	0.0012	0.0413*
	(0.0090)	(0.0060)	(0.0029)	(0.0219)
	280,367	261,163	261,163	74,061
Panel C. Treated: DH autonomy level increased from 2 to 3; Control: DH autonomy level remained 2				
Autonomy level	0.0031	0.0010	0.0030	0.0571**
	(0.0134)	(0.0091)	(0.0040)	(0.0209)
	261,884	244,164	244,164	69,402

Notes: level of significance: ***<0.01, **<0.05, *<0.1. All outcomes are binary indicators and are estimated using linear probability model. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Table 2.8 Effect of dental hygienist autonomy level on dental care utilization for children

Variable	(1) Total Visits	(2) Any Visit	(3) ln(Visits)
Panel A. Treated: DH autonomy level increased from lower levels to higher levels; Control: DH autonomy level remained lower levels			
Autonomy level	0.019	-0.015	0.048**
	(0.053)	(0.014)	(0.022)
	87,909	87,909	36,685
Panel B. Treated: DH autonomy level increased from 2 to higher levels; Control: DH autonomy level remained 2			
Autonomy level	0.061	-0.006	0.056**
	(0.057)	(0.016)	(0.022)
	80,602	80,602	33,572
Panel C. Treated: DH autonomy level increased from 2 to 3; Control: DH autonomy level remained 2			
Autonomy level	0.129*	0.015	0.060*
	(0.070)	(0.017)	(0.034)
	75,530	75,530	31,147

Notes: level of significance: ***<0.01, **<0.05, *<0.1. We estimate a two-part model where the first part is a logit regression using an indicator of any visit and the second part is a log-linear regression. Columns (1) reports the total marginal effect from the two-part model, Column (2) reports the marginal effect from the logit regression, and Columns (3) reports the marginal effect (i.e. coefficient) from the second part. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Table 2.9 Effect of dental hygienist autonomy level on dental care visits by provider type

Variable	(1) Visits to only GD	(2) Visits to only DH	(3) Visits to GD & DH
Panel A. Treated: DH autonomy level increased from lower levels to higher levels; Control: DH autonomy level remained lower levels			
Autonomy level	0.002	-0.001	-0.004
	(0.029)	(0.003)	(0.030)
	308,449	308,449	308,449
Panel B. Treated: DH autonomy level increased from 2 to higher levels; Control: DH autonomy level remained 2			
Autonomy level	-0.014	0.000	0.026
	(0.036)	(0.003)	(0.029)
	280,367	280,367	280,367
Panel C. Treated: DH autonomy level increased from 2 to 3; Control: DH autonomy level remained 2			
Autonomy level	0.017	-0.003	0.038
	(0.034)	(0.004)	(0.040)
	261,884	261,884	261,884

Notes: level of significance: ***<0.01, **<0.05, *<0.1. For each type of visit, we report the total marginal effect from the two-part model where the first part is a logit regression using an indicator of any visit and the second part is a log-linear regression. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Table 2.10 Effect of dental hygienist autonomy level on dental care visits in Health Professional Shortage Areas

Variable	(1) Total Visits	(2) Any Visit	(3) ln(Visits)
Panel A. Treated: DH autonomy level increased from lower levels to higher levels; Control: DH autonomy level remained lower levels			
Autonomy level	0.019	-0.007	0.033**
	(0.028)	(0.007)	(0.015)
	188,685	188,685	68,997
Panel B. Treated: DH autonomy level increased from 2 to higher levels; Control: DH autonomy level remained 2			
Autonomy level	0.017	-0.007	0.030*
	(0.031)	(0.008)	(0.017)
	174,715	174,715	64,304
Panel C. Treated: DH autonomy level increased from 2 to 3; Control: DH autonomy level remained 2			
Autonomy level	0.050	-0.002	0.048**
	(0.046)	(0.013)	(0.022)
	161,965	161,965	58,882

Notes: level of significance: ***<0.01, **<0.05, *<0.1. We estimate a two-part model where the first part is a logit regression using an indicator of any visit and the second part is a log-linear regression. Columns (1) reports the total marginal effect from the two-part model, Column (2) reports the marginal effect from the logit regression, and Columns (3) reports the marginal effect (i.e. coefficient) from the second part. All models include state and year fixed effects. Standard errors in parenthesis are clustered at the state level.

Chapter 3 The Effect of Paid Sick Leave on Worker Absenteeism and Health Care Utilization

3.1 Introduction

Paid sick leave is an employer-provided benefit that allows workers to take time off from work when sick without losing wage income. The provision of paid sick leave is conceptually appealing because it allows workers to seek medical care in a timely manner, which may speed recovery from illness and prevent extended work absences. Paid sick leave can also help prevent the spread of contagious diseases among workers (DeRigne et al., 2016; Pichler and Ziebarth, 2019; Skatun, 2003). As a result, the provision of paid sick leave has the potential to reduce firm productivity loss and improve worker well-being. It is for these reasons that most high-income countries have universal paid sick leave laws requiring employers to provide sick paid leave to their workers (Heymann et al., 2009). However, there is no federal law in the United States that mandates paid sick leave, so access to paid sick leave varies greatly across both geographic areas and industries. Currently, only 68 percent of all private sector workers have paid sick leave as a fringe benefit, and relatively few of these workers are in low wage or part-time jobs (Bureau of Labor Statistics, 2017).[12]

In the absence of federal legislation, some states and localities have taken initiatives to expand access to paid sick leave. By the end of 2017, eight states and the District of Columbia had enacted laws requiring certain employers to provide paid sick leave to eligible employees (Kurani et al., 2017).[13] However, there is a lot of variability across these state mandates in

---

[12] For example, only 35 percent of private sector part-time workers have paid sick leave. Among workers in the lowest decile of the wage distribution, 30 percent have paid sick leave as compared to 92 percent among workers in the highest decile of the distribution (Bureau of Labor Statistics, 2017).

[13] These eight states include Arizona, California, Connecticut, Massachusetts, Oregon, Rhode Island, Vermont, and Washington. For more information on these state laws see http://www.ncsl.org/research/labor-and-employment/paid-sick-leave.aspx, accessed on February 14, 2018.

employer/employee eligibility requirements and the generosity of the benefits. For example, Connecticut was the first state to pass a paid sick leave law (on January 1, 2012), but this law is rather limited in scope: sick leave benefits are mandated for service-sector employees in non-small businesses, who account for only 20 percent of the state's workforce (Pichler and Ziebarth, 2017). Under the Connecticut law, eligible employees only accrue one hour of paid time off for every 40 hours worked and can use no more than 40 hours on paid sick leave unless otherwise allowed by their employers. In contrast, the Healthy Workplaces, Healthy Families Act of 2014 in California mandates that business establishments of all sizes provide one hour of paid sick leave for every 30 hours worked. Twenty eight cities and two counties have also passed laws that aim to increase workers' access to paid sick leave (Kurani et al., 2017), and there has been some recent movement on paid sick leave policy at the federal level. In particular, former President Obama signed an executive order in September 2016 requiring federal contractors to provide up to seven days per year of paid sick leave to their employees (Office of the Press Secretary, 2016).

Notwithstanding the growing popularity of paid sick leave laws, critics of mandatory paid sick leave argue that these laws have unintended adverse effects on labor supply decisions. If a paid sick leave scheme grants employees a pre-specified number of days off during a calendar year, healthy workers who have not exhausted their benefits may wrongfully take days off towards the end of the year. If large enough, this moral hazard effect may offset the firm productivity gains from paid sick leave. Critics also point out that it might be more efficient to let the private market, as opposed to the government, determine the optimal level of paid sick leave benefits (Treble and Barmby, 2011).

Past studies of paid sick leave mostly focus on its labor supply effects. The strongest empirical evidence to date comes from studies using data from European countries. These studies rely almost exclusively on difference-in-differences models to exploit variation in paid sick leave benefits resulting from legislative reforms. They have established a strong and consistent link between paid sick leave benefits and increased incidence and duration of worker absenteeism (Henrekson and Persson, 2004; Johansson and Palme, 2002, 2005; Puhani and Sonderhof, 2010; Ziebarth and Karlsson, 2014).

Due in part to the historical lack of paid sick leave legislation, there are few studies on the effect of paid sick leave in the U.S. Most of the existing studies are descriptive in nature. For example, DeRigne et al. (2016) estimate a set of linear regression models and report that paid sick leave is associated with more sickness absence days and a lower probability of delaying or forgoing medical care. One challenge in the interpretation of these results is that workers who are risk adverse, or have greater preferences for health or medical care, may select jobs that offer paid sick leave. The failure to account for such non-random sorting into jobs with paid sick leave benefits makes it difficult to determine whether difference in labor supply and other outcomes are due to the provision of paid sick leave or to differences in worker characteristics. To address this selection problem, Ahn and Yelowitz (2016) restrict their analysis to individuals in administrative occupations with similar observed individual and firm characteristics and find that paid sick leave increases absenteeism by about 40 percent. They argue that fringe benefits like paid sick leave should be a secondary consideration for workers seeking these types of jobs, which limits the degree of selection, provided that worker attrition is random. That said, their use of cross-sectional variation in paid sick leave benefits makes it difficult to account for unobserved worker characteristics that could be correlated with both access to paid sick leave benefits and the outcome variables.

Other identification approaches include the use of variation in paid sick leave resulting from the staggered implementation of state and local mandates. In particular, Callison and Pesko (2016) find that these mandates reduce both the likelihood of employment and hours worked, while Pichler and Ziebarth (2018) find that paid sick leave mandates did not have any statistically significant effects on either wages or employment. However, in a related study, Pichler and Ziebarth (2017) identify a negative correlation between paid sick leave and regional-level influenza disease rates, which suggests that paid sick leave may reduce presenteeism (i.e. working while sick). The conflicting findings in these studies mainly arise from differences in the construction of treatment and control groups, or the failure to fully account for selection.

In addition to labor market effects, researchers have also examined how paid sick leave affects health care utilization (Bhuyan et al., 2016; Callison and Pesko, 2016; DeRigne et al., 2016; Klein, 2016; Vicente, 2017). Despite methodological differences and the failure in some studies to account for selection, there is a consensus among these studies that paid sick leave is associated with fewer emergency room visits and more office-based medical visits.

In this paper, we seek to provide new empirical evidence on how the provision of paid sick leave benefits affects worker absenteeism and health care utilization. We exploit the longitudinal dimension of the Medical Expenditure Panel Survey (MEPS) using a difference-in-differences matching method (Smith and Todd, 2005) in order to account for selection into jobs that offer paid sick leave. Unlike a standard matching estimator that relies on cross-sectional variation for identification (and could suffer from selection on unobservables), the difference-in-differences matching method accounts for selection along both observed and unobserved dimensions under a set of reasonable assumptions. Our study is closely related to the work by Klein (2016), who also attempts to estimate the effects of paid sick leave using the MEPS. He finds, using fixed effects regressions, that there is no statistically significant change in worker absenteeism after either gaining or losing paid sick leave benefits. At the same time, he finds that gaining and losing paid sick leave benefits are both associated with a statistically significant *decrease* in sickness absence days among individuals with chronic conditions.[14]

The estimates from our preferred specification indicate that access to paid sick leave benefits has statistically significant effects on both worker absenteeism and health care utilization. Specifically, gaining paid sick leave benefits is associated with a 24.7 percent increase in sickness absence days among female workers. We also find a 15.7 percent reduction in sickness absence days among female workers who lost these benefits. Interestingly, we do not find any evidence of increased absenteeism among male workers who gain paid sick leave, although they respond to losing these benefits by reducing the probability of taking sickness absence days by about 10.3 percent. In addition, we find that the probability of outpatient visits increases by 9.9 percent among women who gain paid sick leave while no such relationship is detected for men. However, for both male and female workers we do not find any statistically significant changes in either emergency department visits or self-reported health after gaining or losing paid sick leave. Given that both sickness absence days and outpatient visits by women are responsive to paid sick leave benefits, it is possible that expanding paid sick leave to more women would be welfare improving.

3.2 Data

The data source for this study is the 2000-2013 MEPS, which is a nationally representative survey of the U.S. non-institutionalized population that contains detailed information on health

[14] Klein (2016) argues that the *decrease* in sickness absence days after gaining access to paid sick leave benefits could be due to improved health status.

care utilization and costs.[15] Each year a new panel of individuals enters the survey and is interviewed five times over two calendar years. The MEPS is well-suited for our study due to two unique features. First, the MEPS tracks access to paid sick leave over time for a large number of individuals. This makes it possible for us to exploit the longitudinal dimension of the survey to address the endogeneity problem. Second, the MEPS provides detailed information on the individual's sickness absence from work and utilization of health care services. Furthermore, the MEPS contains a rich set of demographic and employment characteristics that are important determinants of worker's health status and health care utilization. We restrict our sample to individuals aged 18-64 who report a main job in any of the five interview rounds and exclude self-employed workers and full-time students. In order to create a balanced panel, we drop from the sample individuals that have missing data for sickness absence in any of the survey rounds.[16] The number of individuals that meet these requirements is 50,879.

To identify individual's access to paid sick leave, we rely on the following question that is asked in all five rounds of the survey: "On this job, {(do/does)/did} (you/person) have paid time off if (you/person) {(are/is)/ (were/was)} sick." Since we are interested in the effects of both gaining and losing paid sick leave benefits, we construct two separate treatment groups using a similar approach as Klein (2015). Specifically, the first treatment group contains individuals that had no paid sick leave in round 1 and acquired the benefit in any of the subsequent rounds; the second treatment group consists of those who had paid sick leave in round 1 and then lost the benefit in ensuing rounds. In both cases, the control group includes

[15] We do not include the panels before 2000 because the question on sickness absence days was not asked for all rounds for years before 2000.
[16] For this reason, 6,539 of 57,418 observations were dropped from the analysis sample.

workers with paid sick leave for all five rounds ("always takers") as well as those who never had paid sick leave for the entire survey period ("never takers").

The majority of individuals (more than 95 percent) in our two treatment groups had a change in paid sick leave status contemporaneously with a job change. In order to separately identify the effect of paid sick leave from the job change, we further subset our data sample to the set of individuals who experienced a job change. This allows us to difference out the effect of the job change on the outcome variable when estimating the effect of gaining or losing paid sick leave using difference-in-differences matching. However, we exclude workers who changed jobs in rounds 1 and 5 since we do not observe the outcomes after or before the treatment for these individuals. The final estimation sample contains 5,037 individuals, among which 797 are in the treatment group 1 (gaining paid sick leave), 724 are in the treatment group 2 (losing paid sick leave), and the control group contains 3,516 individuals.

The main outcome of interest is sickness absence days, which is constructed based on the MEPS question that asks respondents "the number of times {the person} lost half-day or more from work because of illness, injury, mental or emotional problems" during the interview round.[17] To measure how changes in access to paid sick leave affects health care utilization, we use the number of office-based visits and emergency department visits (ED) in each round. Importantly, since the length of rounds ("reference period") varies across respondents, we standardize all of our outcome measures in each round to a 12-month period.[18] We also investigate the relationship between paid sick leave and both sickness absence days and medical

[17] Following Peng et al. (2016), we construct sickness absence days assuming one full day of work was lost.
[18] To normalize the outcome measures, we divide the number of absence days or medical visits by the length of the reference period in question and then multiply by 365.25. For example, 10 sickness absence days in a reference period of 100 days is normalized to 36.5 days for the full year. One drawback of Klein's (2015) approach is that he did not normalize sickness absence days across rounds, which could result in biased estimates.

visits at the intensive margin by estimating the effect of a dummy variable indicating whether the individual had any sickness absence days or medical visits.

Table 3.1 contains a comparison of individual characteristics for both the treatment and control groups using data from the first MEPS survey round. The demographic characteristics of individuals who experienced a change in paid sick leave status are largely similar to those who did not. The main difference is that individuals in treatment group 1 who gained paid sick leave had lower wages and worked for smaller firms that were less likely to be unionized than the control group.

3.3 Empirical methods

As discussed above, the primary empirical challenge when estimating the causal effect of paid sick leave is self-selection. Individuals who value paid sick leave benefits more highly (due to preferences or worse health status) may seek employment at firms that offer these benefits. Therefore, a standard cross-sectional model, whether it is a parametric regression or a nonparametric matching model, will generally yield biased estimates due to an inability to account for unobserved factors that are correlated with both the provision of paid sick leave and the outcome variable. To address this endogeneity issue in the absence of a credible natural experiment, we employ the difference-in-differences matching estimator proposed by Heckman et al. (1997). Unlike cross-sectional matching estimators that require mean independence for identification (i.e., treatment is as good as randomly assigned conditional on observed variables), the difference-in-differences matching estimator relies on a weaker identifying assumption. Specifically, it allows for time-invariant unobserved differences between treatment and control units, which is in the same spirit as the common trend assumption for a standard difference-in-differences model, but applied at the individual level. This is particularly attractive for our application since we use a short 2-year panel in our main analysis; and one could argue that the main confounder, unobserved health status, is relatively stable over such a short timeframe.

More formally, denote $D_i = 1$ if an individual gains (or loses) paid sick leave benefits and $D_i = 0$ if otherwise (i.e. the "treatment"). Let Y^1 be the outcome of interest if an individual acquires (or loses) paid sick leave and let Y^0 be the same outcome if there is no change in the person's paid sick leave status. We observe two periods t and t' before and after the treatment. For each individual i, we then define the difference in potential outcomes by treatment status as $Y_i^0 = Y_{it'}^0 - Y_{it}^0$ for $D_i = 0$ and $Y_i^1 = Y_{it'}^1 - Y_{it}^1$ for $D_i = 1$. For the estimates to have a causal interpretation, the following "mean-difference independence" condition has to be met for the average treatment effect on the treated:

$$E(Y^0|X, D = 1) = E(Y^0|X, D = 0).$$

Intuitively, this assumption requires that the evolution of outcome in the absence of treatment is similar between the treatment and control groups conditional on a set of observed factors. Following Fan and Jin (2015), we write the estimator for the treatment effect as

$$ATT = \frac{1}{N_1} \sum_{(i|D_i = 1)} \left\{ Y_i^1 - \sum_{(j|j \in C(i))} w_{ij} \hat{Y}_j^0 \right\},$$

where N_1 is the number of treated individuals; $\hat{Y}_0$ is the differenced outcome for matched individuals in the control group; and $C(i)$ represents the set of matched individuals for individual i in the treatment group. Since the number of matches vary across the treatment group, we use a set of weights w_{ij} to construct a single estimate of the counterfactual outcome for each treated person.

To operationalize the nonparametric difference-in-differences matching method, we need to calculate the changes in the outcomes pre- and post-treatment, while accounting for the fact that the timing of treatment varies across individuals. This is further complicated by differences in the length of the MEPS interview rounds. To address both of these issues, we use the following approach to construct the main outcome measures:

$$Y = \log\left(\frac{\sum_{i \in t'} y_i}{\sum_{i \in t'} T_i} \times 365\right) - \log\left(\frac{\sum_{j \in t} y_j}{\sum_{j \in t} T_j} \times 365\right),$$

where y denotes the raw counts of sickness absence days or medical visits (the outcome variables), T denotes the length of the corresponding interview round, and t and t' represent the interview rounds before and after treatment, respectively.

Note that we do not include in these calculations data from the round where the actual treatment occurs since the exact date of job change is not asked in the MEPS. Also, because the distributions of both sickness absence days and medical visits are heavily skewed to the right, we use the difference in the *logarithm* of the normalized outcome to mitigate any undue influence of outliers.

Ideally, we would like to pair each treated individual with a control individual where the values of the matching covariates are identical. However, due to the finite number of potential matches and large number of matching variables, it is not possible to find a sufficient number of exact matches on all covariates. We instead estimate a propensity score using the full set of demographic, socioeconomic, and employment characteristics, including age, race and ethnicity (white and nonwhite), marital status, region (Northeast, South, Midwest, and West), urban residence, years of education, number of children under 5 or 18 in the household, log of income earned by other family members (normalized by household size), presence of chronic conditions,[19] hourly wage, employer size, industry indicators,[20] and occupation indicators (white collar occupations and all other occupations).

The matching algorithm we use in the difference-in-differences estimator is nearest neighbor matching. Specifically, we construct counterfactuals by averaging the outcomes among untreated individuals that are most similar to a particular treated individual based on their propensity scores.[21] Since all of the changes in paid sick leave status occur concurrently with job changes in our estimation sample, we match an individual who experienced a change in paid sick leave and concurrent job change to an untreated individual that had a job change during the same MEPS round. We implement the estimator separately on round 2, 3, and 4 and then take the weighted average of the three estimators. We conduct separate analyses for men and women for all of our outcomes. All standard errors are calculated using 500 bootstrap replications.[22]

3.4 Results

3.4.1Main results

Table 3.2 contains estimates from our difference-in-differences propensity score matching models with three nearest neighbors. Since estimates in columns 1 and 2 are effectively

[19] An individual is coded as chronically ill if the person reported at least one of the following conditions: cancer, diabetes, chronic obstructive pulmonary disease, heart disease, asthma, and anxiety.
[20] The industry indicators include (1) natural resources/mining/construction/manufacturing; (2) professional and business services/education, health, and social services; (3) wholesale and retail trade/transportation and utilities; and (4) other services/public administration/military/ unclassifiable industry.
[21] Under nearest neighbor matching the weights w_{ij} equal the inverse of the number of neighbors matched to individual i. Note that in this case the propensity score is used to determine the quality of the match, whereas in spatial nearest neighbor matching treatment and control observations are matched by their similarity in one or more specific variables (usually individual characteristics).
[22] Typically, one would adjust the standard errors of the estimates for the complex design of the MEPS (i.e. stratification and clustering). In this case, however, very few individuals are contained within the same PSU, so implementing a block bootstrap was not possible. Instead, we used a design effects approach to calculate inflation factors for the standard errors. The inflation factors were calculated by dividing the standard error of the treatment effect in an OLS model with survey adjustments by the standard error of the treatment effect in an OLS model without survey adjustments. Because these inflation factors ranged from 0.98 to 1.02 the significance of the estimates in the difference-in-differences model was not affected.

differences in the log of the outcome variable, they can be interpreted as percentage changes when multiplied by 100. We also estimate intensive margin effects in separate regressions using the difference in the probability of having any sickness absence day and present the results in columns 3 and 4. Several findings emerge from the estimates on sickness absence days. First, gaining paid sick leave is associated with a statistically significant increase of 24.7 percent in sickness absence days among female workers, while losing paid sick leave is associated with a statistically significant decrease of 15.7 percent in sickness absence days. No such effects are detected for male workers (see columns 1 and 2 of Table 3.2). When focusing on the extensive margin, we find that gaining (losing) paid sick leave is associated with a statistically significant increase (decrease) of 13.7 (8.0) percentage points in the probability of having any sickness absence days among female workers, which represents a 32.4 (17.6) percent change when normalized with the overall mean. We also find that losing paid sick leave reduces the probability of having any sickness absence days by 10.3 percentage points (34 percent relative to the mean) among male workers (columns 3 and 4 of Table 3.2). Taken together, these results suggest that absenteeism among female workers is quite responsive to changes in paid sick leave status. Evidence of the responsiveness of sickness absence days to paid sick leave among male workers is mostly lacking, although we do detect a similar response to women of losing paid sick leave at the intensive margin.

Table 3.3 contains difference-in-differences propensity score matching estimates for office-based visits. While our estimates suggest that there is no statistically significant change in the overall number of outpatient visits for women, we do find evidence that gaining paid sick leave increased the probability of having an office-based visit by 10 percentage points (15 percent relative to the mean) among female workers (column 3). Among male workers we continue to find no effect on utilization of outpatient care after either gaining or losing paid sick leave benefits.

3.4.2 Robustness Checks

One potential concern with our empirical approach is that our estimates might be sensitive to the number of neighbors used to construct the counterfactuals. To assess the robustness of our main findings on sickness absence days, we re-estimate the treatment effects using 5 and 10 nearest neighbors in the comparison group for every treated individual. The estimates from this exercise are reported in Table 3.4, and they are largely similar to our main estimates. In addition, we test the sensitivity of our results to alternative matching algorithms by re-estimating our models using the nearest neighbor matching algorithm of Abadie and Imbens (2006, 2011). In this case observations are matched based on the Euclidean distance computed using all matching variables as opposed to the similarity of propensity scores. We use the same set of matching variables as in the propensity score method. One attractive feature of this technique is that it allows us to impose exact matching on job-change round. The estimates from this method are reported in Table 3.5 and are generally consistent with our main estimates.

3.5 Discussion and Conclusion

In this paper, we examine the causal effects of both gaining and losing paid sick leave benefits on absenteeism and health care utilization. Exploiting the longitudinal design of the MEPS data, we use a difference-in-differences matching technique to address the endogeneity of access to paid sick leave benefits. In the absence of exogenous policy changes, our empirical strategy requires a weaker identifying assumption than standard cross-sectional or fixed-effects models.

Our results show that there are asymmetric responses to changes in paid sick leave status. In particular, we find a large increase in absenteeism among female workers after the acquisition of paid sick leave, but a smaller decrease in sickness absence days when they lose the benefit. Interestingly, we do not find any changes in absenteeism among male workers who gained paid sick leave benefits. But they too responded to losing paid sick leave benefits by cutting back sickness absence days by nearly the same amount as women. The large increase in sickness absence days among female workers may reflect underlying medical needs that are greater than those for men. It is also possible that women are more likely than men to take their children to medical appointments. Unfortunately, our sample size isn't large enough to verify this hypothesis empirically.

We investigate the welfare implications of the provision of paid sick leave through a careful examination of health care utilization. On the one hand, offering workers paid sick leave could be welfare-improving if it leads to more prompt use of medical care and improves health outcomes. On the other hand, there is potential for moral hazard if workers take unnecessary days off for reasons other than resting or seeking medical care. Our finding that gaining paid sick leave increases the probability of having an office-based visit among women is consistent with improved access to prompt medical care. To further investigate this issue, we break down office-based visits into weekday and weekend visits using the information on the visit date for each medical record. Columns 1-4 of Table 3.6 present the estimates from the difference-in-differences propensity score matching model on indicators of any weekday visit and any weekend visit. These estimates suggest that gaining paid sick leave is associated with a statistically significant increase of 12.4 percentage points (22%) in the probability of having a weekday office-based visit among women, while no effects are detected for weekend visits. In addition, we find no evidence of any statistically significant changes in the probability of emergency department visits or self-reported health status for both men and women (see columns 5-8).[23] Taken together, our results provide suggestive evidence that gaining paid sick leave could be welfare-improving for female workers, but a more definitive welfare statement cannot be made without fully assessing long term health outcomes.[24]

Our study is subject to several limitations. First, our estimates represent the immediate effects of gaining or losing paid sick leave; and as a result, we are not able to examine the long-term effects of having paid sick leave benefits. It is possible that the responsiveness of sickness absence days to paid sick leave benefits is larger when workers have had time to accumulate sick day balances. Similarly, we cannot evaluate the impact of the availability of paid sick leave benefits over an extended period of time on health status, which is necessary in order to fully assess the welfare effects of paid sick leave benefits. One main argument for extending the benefit to more individuals is that it improves access to care and eventually leads to better health outcomes. While we do find better access to outpatient care by women with paid sick leave, we cannot verify whether this leads to better overall health. Finally, we do not have information on the exact number of paid sick days granted to workers, which effectively limits our analysis to the extensive margin. However, the effect at the intensive margin is equally important when designing policies that mandate employers to provide certain number of paid sick days to their employees.

[23] We construct an indicator variable for self-reported health status being very good or good.

[24] Both men and women who lost paid sick leave benefits may have forgone rest time or medical care as a result of presenteeism. Although the estimated effects of losing benefits on medical visits are negative for both men and women, none of them are statistically significant. However, we caution that the imprecision could also be due to the lack of statistical power in our small samples.

Despite these limitations, our results have important policy implications. For state policymakers who are concerned with the effectiveness of paid sick leave mandates, the findings suggest that female workers' decisions to take time off from work are very responsive to the availability of (and lack thereof) paid sick leave. Furthermore, at least some of this time is devoted to formal medical treatment, which could increase the speed of recovery and have subsequent productivity benefits. However, additional research using larger samples of male workers is needed to determine whether our lack of findings for men is due to low statistical power or reflects differential demand for sickness absence days between men and women.

Table 3.1 Descriptive statistics from survey round 1

	Treatment group 1 (gained PSL)		Treatment group 2 (lost PSL)		Control group (no change in PSL)	
Variables	Mean	S.D.	Mean	S.D.	Mean	S.D.
Outcomes						
Change in log of absence days	0.014	1.299	-0.398	1.338	-0.160	1.236
Change in log of office-based visits	0.068	0.977	-0.148	1.110	-0.028	1.011
Change in log of ER visits	0.001	0.336	-0.023	0.365	0.007	0.306
Personal characteristics						
Female	0.486	0.500	0.446	0.497	0.446	0.497
Male	0.514	0.500	0.554	0.497	0.554	0.497
Age	34.873	10.026	36.65	10.76	36.62	10.62
Married	0.473	0.500	0.555	0.497	0.555	0.497
Years of education	13.179	2.833	12.919	3.213	13.258	3.372
Urban residence	0.849	0.358	0.828	0.377	0.838	0.368
White	0.444	0.497	0.474	0.500	0.466	0.499
Non-white*	0.556	0.497	0.526	0.500	0.534	0.499
Northeast	0.138	0.350	0.130	0.332	0.140	0.347
Midwest	0.205	0.404	0.232	0.422	0.208	0.406
South	0.405	0.491	0.406	0.491	0.391	0.488
West*	0.252	0.435	0.232	0.422	0.260	0.439
Number of children under 5	0.320	0.637	0.380	0.666	0.363	0.658
Number of children aged 6-17	0.588	0.921	0.640	0.958	0.630	0.964
Private insurance	0.522	0.500	0.771	0.421	0.669	0.471
Chronically ill	0.191	0.393	0.211	0.409	0.200	0.400
Log of family income per capita	9.997	0.828	9.982	0.944	10.072	0.892
Job characteristics						
Employer size	90.104	151.76	127.54	170.04	116.6	168.8
Union	0.040	0.196	0.076	0.265	0.068	0.252
Hourly wage	13.014	8.113	15.028	8.490	17.06	11.06
White collar occupation	0.570	0.495	0.597	0.491	0.594	0.491
Other occupation*	0.430	0.495	0.403	0.491	0.406	0.491
Industry—construction and manufacturing	0.130	0.337	0.174	0.379	0.168	0.374

Industry—professional and education	0.161	0.368	0.178	0.383	0.133	0.339
Industry—transportation and utility	0.450	0.498	0.418	0.493	0.458	0.498
Industry—other*	0.048	0.213	0.067	0.251	0.058	0.233
Observations	797		724		3,516	

*Reference group. Details of industry indicators are specified in the text.

Table 3.2 Propensity score matching estimates of the effect of paid sick leave on sickness absence days

	Sickness absence days		Sickness absence days>0	
	(1)	(2)	(3)	(4)
Panel A. men				
Gained paid sick leave	-0.036		-0.005	
	(0.070)		(0.041)	
Lost paid sick leave		-0.105		-0.103***
		(0.068)		(0.039)
N	1930	1907	2357	2348
Panel B. women				
Gained paid sick leave	0.247**		0.137***	
	(0.100)		(0.050)	
Lost paid sick leave		-0.157*		-0.080*
		(0.095)		(0.046)
N	1677	1610	1955	1891

Notes: level of significance: ***p<0.01, **p<0.05, *p<0.1; standard errors are based on 500 bootstrap replications. The number of nearest neighbors is 3.

Table 3.3 Propensity score matching estimates of the effect of paid sick leave on office-based medical visits

	Office-based Visits		Office-based Visits >0	
	(1)	(2)	(3)	(4)
Panel A. men				
Gained paid sick leave	0.038		0.015	
	(0.053)		(0.041)	
Lost paid sick leave		-0.088		-0.060
		(0.067)		(0.038)
N	1930	1907	2357	2348
Panel B. women				
Gained paid sick leave	0.098		0.099**	
	(0.070)		(0.040)	
Lost paid sick leave		-0.074		-0.042
		(0.072)		(0.044)
N	1677	1610	1955	1891

Notes: level of significance: ***p<0.01, **p<0.05, *p<0.1; standard errors are based on 500 bootstrap replications. The number of nearest neighbors is 3.

Table 3.4 Propensity score matching estimates of the effect of paid sick leave on sickness absence days (for NN=5 and NN=10)

	NN=5				NN=10			
	Sickness absence days		Sickness absence days>0		Sickness absence days		Sickness absence days>0	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A. men								
Gained PSL	-0.006		-0.012		0.011		-0.018	
	(0.072)		(0.038)		(0.066)		(0.036)	
Lost PSL		-0.089		-0.101***		-0.089		0.096***
		(0.063)		(0.038)		(0.062)		(0.036)
N	1930	1907	2357	2348	1930	1907	2357	2348
Panel B. women								
Gained PSL	0.220**		0.129***		0.229**		0.149***	
	(0.097)		(0.048)		(0.091)		(0.045)	
Lost PSL		-0.140		-0.097**		-0.123		-0.098**
		(0.091)		(0.043)		(0.088)		(0.042)
N	1677	1610	1955	1891	1677	1610	1955	1891

Notes: level of significance: ***p<0.01, **p<0.05, *p<0.1; standard errors are based on 500 bootstrap replications; NN: number of nearest neighbors.

Table 3.5 Nearest neighbor matching estimates of the effects of paid sick leave

	Sickness absence days		Sickness absence days>0		Office-based Visits		Office-based Visits>0	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A. men								
Gained PSL	0.010		-0.016		0.057		-0.0101	
	(0.045)		(0.032)		(0.039)		(0.032)	
Lost PSL		-0.104		-0.098***		-0.028		-0.050
		(0.067)		(0.030)		(0.064)		(0.033)
N	2,174	2,142	2,860	2,837	2,174	2,142	2,860	2,837
Panel B. women								
Gained PSL	0.151**		0.104***		0.050		0.099***	
	(0.074)		(0.037)		(0.051)		(0.032)	
Lost PSL		-0.093		-0.092**		-0.076		-0.048
		(0.071)		(0.039)		(0.055)		(0.034)
N	1,925	1,853	2,509	2,419	1,925	1,853	2,509	2,419

Notes: level of significance: ***p<0.01, **p<0.05, *p<0.1. Number of nearest neighbors is 3.

Table 3.6 Propensity score matching estimates of the effects of paid sick leave on weekday and weekend office-based visits and emergency department (ED) visits

	Office-based visits on weekdays>0		Office-based visits on weekends>0		ER Visit>0		Self-reported health	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. men								
Gained paid sick leave	0.034		0.020		-0.011		0.007	
	(0.039)		(0.023)		(0.022)		(0.014)	
Lost paid sick leave		-0.058		0.014		-0.029		0.008
		(0.038)		(0.022)		(0.024)		(0.012)
N	2357	2348	2357	2348	2357	2348	2357	2384
Panel B. women								
Gained paid sick leave	0.124***		-0.016		0.013		0.014	
	(0.040)		(0.038)		(0.031)		(0.020)	
Lost paid sick leave		-0.053		-0.018		-0.021		0.006
		(0.045)		(0.032)		(0.033)		(0.018)
N	1955	1891	1955	1891	1955	1891	1955	1891

Notes: level of significance: ***p<0.01, **p<0.05, *p<0.1; standard errors are based on 500 bootstrap replications; number of nearest neighbors is 3. The measure for self-reported heath is an indicator for whether an individual reports his/her health status as very healthy or healthy.

References

Abadie, Alberto and Guido W. Imbens. 2006. "Large Sample Properties of Matching Estimators for Average Treatment Effects." *Econometrica* 74(1):235–67.

Abadie, Alberto and Guido W. Imbens. 2011. "Bias-Corrected Matching Estimators for Average Treatment Effects." *Journal of Business and Economic Statistics* 29(1):1–11.

Ahn, Thomas and Aaron Yelowitz. 2016. "Paid Sick Leave and Absenteeism: The First Evidence from the U.S." SSRN. https://ssrn.com/abstract=2740366 or http://dx.doi.org/10.2139/ssrn.2740366.

Backhaus, Ramona, Hilde Verbeek, Erik van Rossum, Elizabeth Capezuti, and Jan P. H. Hamers Hamers. 2014. "Nurse Staffing Impact on Quality of Care in Nursing Homes: A Systematic Review of Longitudinal Studies." *Journal of the American Medical Directors Association* 15:383–93.

Bhuyan, Soumitra S., Yang Wang, Jay Bhatt, S. Edward Dismuke, Erik L. Carlton, Dan Gentry, Chad LaGrange, and Cyril F. Chang. 2016. "Paid Sick Leave Is Associated with Fewer ED Visits among US Private Sector Working Adults." *The American Journal of Emergency Medicine* 34(5):784–89.

Bureau of Labor Statistics. 2017. *Employee Benefits in the United States - March 2017*. https://www.bls.gov/news.release/pdf/ebs2.pdf.

Callison, Kevin and Michael F. Pesko. 2016. "The Effect of Mandatory Paid Sick Leave Laws on Labor Market Outcomes, Health Care Utilization, and Health Behaviors." Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. https://doi.org/10.17848/wp16-265.

Cameron, Colin A. and Douglas L. Miller. 2015. "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources* 50(2):317–72.

Cawley, John, David C. Grabowski, and Richard A. Hirth. 2006. "Factor Substitution in Nursing Homes." *Journal of Health Economics* 25(2):234–47.

Centers for Medicare and Medicaid Services. 2017. "Design for Nursing Home Compare Five Star Quality System: Technical Users' Guide." Retrieved (http://www.cms.gov/CertificationandComplianc/Downloads/usersguide.pdf).

Department of Health and Human Services. 2002. "Nurse Aide Training." (November). Retrieved (http://www.oig.hhs.gov).

DeRigne, Lea Anne, Patricia Stoddard-Dare, and Linda Quinn. 2016. "Workers without Paid Sick Leave Less Likely to Take Time off for Illness or Injury Compared to Those with Paid Sick Leave." *Health Affairs* 35(3):520–27.

Duan, Naihua, Willard G. Manning, Carl N. Morris, and Joseph P. Newhouse. 1984. "Choosing between the Sample-Selection Model and the Multi-Part Model." *Journal of Business & Economic Statistics* 2(3):283–89.

Fan, Maoyong and Yanhong Jin. 2015. "The Supplemental Nutrition Assistance Program and Childhood Obesity in the United States: Evidence from the National Longitudinal Survey of

Youth 1997.” *American Journal of Health Economics* 1(4):432–60.

Han, Kihye, Alison M. Trinkoff, Carla L. Storr, Nancy Lerner, Meg Johantgen, and Kyungsook Gartrell. 2014. “Associations between State Regulations, Training Length, Perceived Quality and Job Satisfaction among Certified Nursing Assistants: Cross-Sectional Secondary Data Analysis.” *International Journal of Nursing Studies* 51(8):1135–41.

Harrington, Charlene. 2010. “Nursing Home Staffing Standards in State Statutes and Regulations.” Retrieved (https://theconsumervoice.org/uploads/files/issues/Harrington-state-staffing-table-2010.pdf).

Heckman, James J., Hidehiko Ichimura, and Petra E. Todd. 1997. “Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme.” *The Review of Economic Studies* 64(4):605–54.

Henrekson, Magnus and Mats Persson. 2004. “The Effects on Sick Leave of Changes in the Sickness Insurance System.” *Journal of Labor Economics* 22(1):87–113.

Hernandez-Medina, Esther, Susan Eaton, Donna Hurd, and Alan White. 2006. “Training Programs for Certified Nursing Assistants.” AARP Public Policy Institute. https://assets.aarp.org/rgcenter/il/2006_08_cna.pdf.

Heymann, Jody, Hye Jin Rho, John Schmitt, and Alison Earle. 2009. “Contagion Nation: A Comparison of Paid Sick Day Policies in 22 Countries.” *Center for Economic and Policy Research* (May):1–20.

Howe, Erin E. 2014. “Empowering Certified Nurse’s Aides to Improve Quality of Work Life through a Team Communication Program.” *Geriatric Nursing (New York, N.Y.)* 35(2):132–36.

Iowa CareGivers Association. 2004. “Survey of Nurse Aide Registries (Direct Care Worker) in the United States.” http://www.iowacaregivers.org/uploads/pdf/99165icgafullbook.pdf.

Johansson, Per and Mårten Palme. 2002. “Assessing the Effect of Public Policy on Worker Absenteeism.” *The Journal of Human Resources* 37(2):381–409.

Johansson, Per and Mårten Palme. 2005. “Moral Hazard and Sickness Insurance.” *Journal of Public Economics* 89(9):1879–90.

Kaiser Family Foundation. 2018. “Nursing Facilities , Staffing , Residents and Facility Deficiencies , 2009 Through 2016.” http://files.kff.org/attachment/REPORT-Nursing-Facilities-Staffing-Residents-and-Facility-Deficiencies-2009-2016.

Klein, Ryan Soencer. 2016. “Evaluating Health and Healthcare Use Effects of Changes in Paid Sick Leave Access for Workers in the United States.” University of Minnesota.

Kleiner, Morris M. 2016. “Battling over Jobs: Occupational Licensing in Health Care.” *American Economic Review* 106(5):165–70.

Kleiner, Morris M. and Alan B. Krueger. 2013. “Analyzing the Extent and Influence of Occupational Licensing on the Labor Market.” *Journal of Labor Economics* 31(S1):S173–202.

Kleiner, Morris M. and Robert T. Kudrle. 2000. "Does Regulation Affect Economic Outcomes? The Case of Dentistry." *Journal of Law and Economics* 43(October):547–82.

Kleiner, Morris M., Allison Marier, Kyoung Won Park, and Coady Wing. 2016. "Relaxing Occupational Licensing Requirements: Analyzing Wages and Prices for a Medical Service." *Journal of Law and Economics* 59(May):1–42.

Kleiner, Morris M. and Kyoung Won Park. 2010. "Battles Among Licensed Occupations: Analyzing Government Regulations on Labor Market Outcomes for Dentists and Hygienists." *NBER Working Paper* 1–40.

Konetzka, R. Tamara, Karen B. Lasater, Edward C. Norton, and Rachel M. Werner. 2018. "Are Recessions Good for Staffing in Nursing Homes?" *American Journal of Health Economics* 4(4):411–32.

Koppelman, Jane, Kelly Vitzthum, and Lisa Simon. 2016. "Expanding Where Dental Therapists Can Practice Could Increase Americans' Access To Cost-Efficient Care." *Health Affairs* 35(12):2200–2206.

Kurani, Nisha, Usha Ranji, Alina Salganicoff, and Matthew Rae. 2017. "Paid Family Leave and Sick Days in the U.S.: Findings from the 2017 Kaiser/HRET Employer Health Benefits Survey." Kaiser Family Foundation. https://www.kff.org/womens-health-policy/fact-sheet/paid-family-leave-and-sick-days-in-the-u-s-findings-from-the-2017-kaiserhret-employer-health-benefits-survey/.

Langelier, Margaret, Bridget Baker, and Moore Continelli. 2016. *A Dental Hygiene Professional Practice Index by State, 2014*. Rensselaer, NY: Oral Health Workforce Research Center, Center for Health Workforce Studies, School of Public Health, SUNY Albany.

Langelier, Margaret, Tracey Continelli, Jean Moore, Bridget Baker, and Simona Surdu. 2016. "Expanded Scopes of Practice for Dental Hygienists Associated with Improved Oral Health Outcomes for Adults." *Health Affairs* 35(12):2207–15.

Lin, Haizhen. 2014. "Revisiting the Relationship between Nurse Staffing and Quality of Care in Nursing Homes: An Instrumental Variables Approach." *Journal of Health Economics* 37(1):13–24.

Lu, Susan F. 2012. "Product Quality: Evidence from Nursing Homes." *Journal of Economics and Management Strategy* 21(3):673–705.

Lu, Susan Feng and Lauren Xiaoyuan Lu. 2017. "Do Mandatory Overtime Laws Improve Quality? Staffing Decisions and Operational Flexibility of Nursing Homes." *Management Science* 63(11):3566–85.

Manning, W. G. 1998. "The Logged Dependent Variable, Heteroscedasticity, and the Retransformation Problem." *Journal of Health Economics* 17(3):283–95.

Manski, Richard J., Diane Hoffmann, and Virginia Rowthorn. 2015. "Increasing Access to Dental and Medical Care by Allowing Greater Flexibility in Scope of Practice." *American Journal of Public Health* 105(9):1755–62.

Markowitz, Sara, E. Kathleen Adams, Mary Jane Lewitt, and Anne L. Dunlop. 2017.

“Competitive Effects of Scope of Practice Restrictions: Public Health or Public Harm?” *Journal of Health Economics* 55:201–18.

Matsudaira, Jordan D. 2014. “Government Regulation and the Quality of Healthcare: Evidence from Minimum Staffing Legislation for Nursing Homes.” *Journal of Human Resources* 49(1):32–72.

Mertz, Elizabeth A. 2016. “The Dental–Medical Divide.” *Health Affairs* 35(12):2168–75.

Meyerhoefer, Chad D., Irina Panovska, and Richard J. Manski. 2016. “Projections Of Dental Care Use Through 2026: Preventive Care To Increase While Treatment Will Decline.” *Health Affairs* 35(12):2183–89.

Nakrem, Sigrid, Anne Guttormsen Vinsnes, Gene E. Harkless, Bård Paulsen, and Arnfinn Seim. 2009. “Nursing Sensitive Quality Indicators for Nursing Home Care: International Review of Literature, Policy and Practice.” *International Journal of Nursing Studies* 46(6):848–57.

National Center for Health Workforce Analyses. 2004. “Nursing Aides , Home Health Aides , and Related Health Care Occupations -- National and Local Workforce Shortages and Associated Data Needs.” https://bhw.hrsa.gov/sites/default/files/bhw/RNandHomeAides.pdf.

Office of Evaluation and Inspections. 2002. “State Nurse Aide Training: Program Information And Data.” https://oig.hhs.gov/oei/reports/oei-05-01-00031.pdf.

Office of the Press Secretary. 2016. “Fact Sheet: Helping Working Americans Get Ahead by Expanding Paid Sick Leave and Fighting for Equal Pay.” https://obamawhitehouse.archives.gov/the-press-office/2016/09/29/fact-sheet-helping-working-americans-get-ahead-expanding-paid-sick-leave.

Paraprofessional Healthcare Institute. 2019. “Nursing Assistant Training Requirements by State.” Retrieved June 11, 2019 (https://phinational.org/advocacy/nurse-aide-training-requirements-state-2016/).

Peng, Lizhong, Chad D. Meyerhoefer, and Samuel H. Zuvekas. 2016. “The Short-Term Effect of Depressive Symptoms on Labor Market Outcomes.” *Health Economics* 25(10):1223–38.

Pennington, Karen, Jill Scott, and Kathy Magilvy. 2003. “The Role of Certified Nursing Assistants in Nursing Homes.” *Journal of Nursing Administration* 33(11):578–84.

Perry, John J. 2009. “The Rise and Impact of Nurse Practitioners and Physician Assistants on Their Own and Cross-Occupation Incomes.” *Contemporary Economic Policy* 27(4):491–511.

Pichler, Stefan and Nicolas R. Ziebarth. 2017. “The Pros and Cons of Sick Pay Schemes: Testing for Contagious Presenteeism and Noncontagious Absenteeism Behavior.” *Journal of Public Economics* 156:14–33.

Pichler, Stefan and Nicolas R. Ziebarth. 2018. “Labor Market Effects of U.S. Sick Pay Mandates.” *Journal of Human Resources*.

Pichler, Stefan and Nicolas R. Ziebarth. 2019. “Reprint of: The Pros and Cons of Sick Pay

Schemes: Testing for Contagious Presenteeism and Noncontagious Absenteeism Behavior." *Journal of Public Economics* 171:86–104.

Puhani, Patrick A. and Katja Sonderhof. 2010. "The Effects of a Sick Pay Reform on Absence and on Health-Related Outcomes." *Journal of Health Economics* 29(2):285–302.

Reinders, Jan J., Wim P. Krijnen, Pieter Onclin, Cees P. van der Schans, and Boudewijn Stegenga. 2017. "Attitudes among Dentists and Dental Hygienists towards Extended Scope and Independent Practice of Dental Hygienists." *International Dental Journal* 67(1):46–58.

RTI International. 2012. "MDS 3.0 Quality Measures User's Manual." https://www.cms.gov/Medicare/Quality-Initiatives-Patient-Assessment-Instruments/NursingHomeQualityInits/downloads/MDS30QM-Manual.pdf.

Skatun, John Douglas. 2003. "Take Some Days off, Why Don't You? - Endogenous Sick Leave and Pay." *Journal of Health Economics* 22(3):379–402.

Smith, Jeffrey A. and Petra E. Todd. 2005. "Does Matching Overcome LaLonde's Critique of Nonexperimental Estimators?" *Journal of Econometrics* 125(1):305–53.

Stange, Kevin. 2014. "How Does Provider Supply and Regulation Influence Health Care Markets? Evidence from Nurse Practitioners and Physician Assistants." *Journal of Health Economics* 33(1):1–27.

Timmons, Edward, Jason Hockenberry, and Christine Durrance. 2016. "More Battles Among Licensed Occupations: Estimating the Effects of Scope of Practice and Direct Access on the Chiropractic, Physical Therapist, and Physician Labor Market." *Mercatus Research*.

Timmons, Edward Joseph. 2017. "The Effects of Expanded Nurse Practitioner and Physician Assistant Scope of Practice on the Cost of Medicaid Patient Care." *Health Policy* 121(2):189–96.

Traczynski, Jeffrey and Victoria Udalova. 2018. "Nurse Practitioner Independence, Health Care Utilization, and Health Outcomes." *Journal of Health Economics* 58:90–109.

Treble, John and Tim Barmby. 2011. *Worker Absenteeism and Sick Pay*. Cambridge University Press.

Trinkoff, Alison M., Carla L. Storr, Meg Johantgen, Nancy Lerner, Kihye Han, and Kathleen McElroy. 2013. "State Regulatory Oversight of Certified Nursing Assistants and Resident Outcomes." *Journal of Nursing Regulation* 3(4):53–59.

Trinkoff, Alison M., Carla L. Storr, Nancy B. Lerner, Bo Kyum Yang, and Kihye Han. 2017. "CNA Training Requirements and Resident Care Outcomes in Nursing Homes." *Gerontologist* 57(3):501–8.

Tyler, Denise A., Hye Young Jung, Zhanlian Feng, and Vincent Mor. 2010. "Prevalence of Nursing Assistant Training and Certification Programs within Nursing Homes, 1997-2007." *Gerontologist* 50(4):550–55.

Vicente, Korvin. 2017. "The Effect of Paid Sick Leave on Physician Office- Based Visits." CUNY Academic Works. http://academicworks.cuny.edu/hc_sas_etds/217.

Wanchek, Tanya. 2010. “Dental Hygiene Regulation and Access to Oral Healthcare: Assessing the Variation across the US States.” *British Journal of Industrial Relations* 48(4):706–25.

Wing, Coady and Allison Marier. 2014. “Effects of Occupational Regulations on the Cost of Dental Services: Evidence from Dental Insurance Claims.” *Journal of Health Economics* 34(1):131–43.

Wing, Paul, Margaret Langelier, Tracey Continelli, and Ann Battrell. 2005. “A Dental Hygiene Professional Practice Index (DHPPI) and Access to Oral Health Status and Service Use in the United States.” *Journal of Dental Hygiene : JDH / American Dental Hygienists’ Association* 79(2):10.

Ziebarth, Nicolas R. and Martin Karlsson. 2014. “The Effects of Expanding the Generosity of the Statuory Sickness Insurance System.” *Journal of Applied Econometrics* 29:208–30.

Jie Chen

621 Taylor St, Bethlehem, PA 18015
jic412@lehigh.edu
https://sites.google.com/a/lehigh.edu/jiechen/

EDUCATION

Lehigh University, Bethlehem, PA — 2014-2019
Ph.D. in Economics

Lehigh University, Bethlehem, PA — 2012-2014
M.S. in Economics

Peking University, Beijing, China — 2007-2011
B.A. in Economics & Art Conservation

DISSERTATION

Essays on Occupational Regulation and Paid Sick Leave
Committee: Prof. Chad Meyerhoefer (Chair), Prof. Robert Thornton, Prof. Seth Richards-Shubik, Prof. Edward Timmons

RESEARCH FIELDS

Primary: Health Economics, Labor Economics,
Secondary: Applied Econometrics, Public Policy

PUBLICATION

Meyerhoefer, C., S. Sherer, M. Deily, S. Chou, X. Guo, **J. Chen**, M. Sheinberg and D. Levick. Provider and patient satisfaction with the integration of ambulatory and hospital EHR systems, *Journal of the American Medical Informatics Association*, 25(8), August 2018, pp. 1054–1063. (Covered by *Healthcare Finance* and *For The Record Magazine*)

WORKING PAPERS

"The Effects of Dental Hygienist Scope of Practice and Autonomy on Dental Care Utilization" (with C. Meyerhoefer and E. Timmons), *Job Market Paper*

"The Effect of Paid Sick Leave on Worker Absenteeism and Health Care Utilization" (with C. Meyerhoefer and L. Peng), *revise and resubmit*

"Do Training Requirements of Certified Nursing Assistants Affect Staffing Decisions and Quality of Care in Nursing Homes?"

"Patient Selection Under Incomplete Case Mix Adjustment: Evidence from the Hospital Value-based Purchasing Program" (with L. Peng and C. Meyerhoefer)

"The Effect of Housing Price Changes on Labor Market Activity: Evidence from Urban China"

RESEARCH EXPERIENCE

Research Fellow 2017-2019
Knee Center for the Study of Occupational Regulation (CSOR), Loretto, PA

Research Assistant
- Department of Economics, Lehigh University 2014-2016
- Department of Marketing, Lehigh University 2013-2014
- Department of Management, Lehigh University 2012-2014

TEACHING EXPERIENCE (Lehigh)

Adjunct Professor (Instructor)
- *Applied Microeconomic Analysis* Summer 2018

Teaching Assistant
- *Money, Banking, and Financial Markets* Spring & Fall 2016
- *Principles of Economics* Fall 2015
- *Statistical Methods* Spring 2014
- *Lehigh in Shanghai* Summer 2013

ACTIVITIES

Conference Presentations:
- Labor and Employment Relations Association 70th Annual Meeting, Baltimore, MD, June 2018
- Federal Reserve Bank of Chicago, Chicago, IL, September 2018
- Eastern Economics Association Conference, New York, NY, March 2019
- SFU licensing conference, Loretto, PA, April 2019
- American Society of Health Economists (ASHEcon), DC, June 2019

Peer-Reviewing:
- The Journal of Mental Health Policy and Economics

AWARDS

Fellowship, Knee Center for the Study of Occupational Regulation 2017-19
Teaching Assistantship, Lehigh University 2015-16
University Fellowship, Lehigh University 2014-15
Research Assistantship, Lehigh University 2012-14
Teacher Development Certificate, Lehigh University 2014

AFFILIATIONS American Economic Association, Labor and Employment Relations Association, American Society of Health Economists

SKILLS

Softwares: STATA, SAS, SQL, Python, R, Tableau, LaTeX
Languages: English, Mandarin

REFERENCES

Chad D. Meyerhoefer
Arthur F. Searing Professor of Economics
Research Associate, National Bureau of Economic Research
College of Business and Economics
Lehigh University
621 Taylor Street, Bethlehem, PA 18015
Email: chm308@lehigh.edu
(Tel) 610-758-3445

Edward Timmons
Professor of Economics
Director, The Knee Center for the Study of Occupational Regulation
Shields School of Business
Saint Francis University
Schwab Hall 314
P.O. Box 600, Loretto, PA 15940
Email: ETimmons@francis.edu
(Tel) 814-472-3073

Judith A. McDonald (Teaching)
Professor of Economics
College of Business and Economics
Lehigh University
621 Taylor Street, Bethlehem, PA 18015
Email: djm0@lehigh.edu
(Tel) 610-758-5345